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"SOMETHING CURIOUS IN THE MEDICAL LINE"

REGINALD FITZ

In the early days of 1799, Dr. Benjamin Waterhouse of Cambridge, Massachusetts, received a present from Dr. John C. Lettsom, an acquaintance of his in London. Dr. Waterhouse, as everyone knows, was the first Hersean Professor of the Theory and Practice of Physic in the Harvard Medical School, a Quaker from Rhode Island and an individual fairly bristling with peculiarities. He was a man of strong character, a properly cantankerous soul such as Harvard likes to develop from time to time, by no means the kind of a person whom the students could ornament with an affectionate, friendly nickname like "Kitty" [1] or "Copey" or "Frisky" but rather the sort of professor who was held in great awe. Certain mischievous undergraduates once announced that he was prepared to offer a series of public lectures on "Oudenology," [2] which suggests that a number of them considered him unduly diffused and long-winded. On the other hand, one can be sure that after having heard him speak on the evils of tobacco no undergraduates would wish to be caught by him near the College while they were taking a surreptitious pull at a cigar or when they were at all under the influence of ardent spirits.

[1] To non-Harvard men, Professors G. L. Kittredge, C. T. Copeland, and R. B. Merriman.

[2] Liddell and Scott: οὐδενέια, *nothingness.*

Dr. Waterhouse had been educated abroad and had many more friends in England than in America. He kept up his British affiliations by correspondence, for he was a great letter-writer and in the habit of exchanging all manner of ideas with transatlantic friends. Perhaps this year, therefore, Dr. Lettsom meant this particular gift to reach Cambridge in time for Christmas. In any event it arrived at Dr. Waterhouse's home shortly after New Year's Day and was an interesting present for any doctor to receive—a copy of Dr. Edward Jenner's [3] "An Inquiry into the Causes and Effects of the Variolae Vaccinae," fresh from the press, as interesting to read now as it was a hundred and forty years ago. It tells for the first time that 'what renders the cow-pox virus so extremely singular is, that the person who has been thus affected is for ever secure from the infection of the Small Pox' and how the latter unpleasant disease can be prevented by the simple expedient of vaccination.

Dr. Waterhouse studied this article with the greatest zeal. He was familiar with smallpox and its ravages, he knew about Zabdiel Boylston and inoculation, but here seemed something vastly more promising. After reading and re-reading it he soon determined that it should be publicized. So in the Columbian Sentinel for March 16, 1799 there appeared from his pen a short newsletter under the caption "Something curious in the Medical Line," [4] describing Jenner's work as simply and clearly as possible. Dr. Waterhouse says that this article shared the fate of most articles dealing with medical discoveries. A few of his friends read it carefully and were prepared to accept Jenner's work as an important scientific contribution, highly interesting to humanity; more, being conservative Bostonians, said nothing: they observed that wise and prudent conduct which would allow them to condemn or applaud the discovery as events might later prove; most of those who glanced through the article, however, openly ridiculed the thought of producing cow-pox to prevent smallpox and considered the idea one of Professor

[3] This tract quickly achieved great popularity and was soon translated into German, Latin, and French. The copy sent to Dr. Waterhouse was the first to cross the Atlantic Ocean.

[4] His enemies called this the first of a series of advertisements and claimed that Waterhouse was unethical to use the public press for medical writings.

AN

INQUIRY

INTO

THE CAUSES AND EFFECTS

OF

THE VARIOLÆ VACCINÆ,

A DISEASE

DISCOVERED IN SOME OF THE WESTERN COUNTIES OF ENGLAND,

PARTICULARLY

GLOUCESTERSHIRE,

AND KNOWN BY THE NAME OF

THE COW POX.

BY EDWARD JENNER, M.D. F.R.S. &c.

——— QUID NOBIS CERTIUS IPSIS
SENSIBUS ESSE POTEST, QUO VERA AC FALSA NOTEMUS.

LUCRETIUS.

London:

PRINTED, FOR THE AUTHOR,

BY SAMPSON LOW, N°. 7, BERWICK STREET, SOHO:

AND SOLD BY LAW, AVE-MARIA LANE; AND MURRAY AND HIGHLEY, FLEET STREET.

1798.

Fig. 1.
Title page of Jenner's original publication on Vaccination. 1798.

Waterhouse's medical whims which might arrive today and tomorrow would be no more.

Public interest in this trial balloon was anything but impressive. So next Dr. Waterhouse determined to approach a more discerning audience. At one of the meetings of the American Academy of Arts and Sciences held in the Philosophy Chamber of Harvard Hall during the ensuing winter, he says that he told the members present what he had read of the kine pock as a preventive against smallpox, and that he exhibited Dr. Jenner's publication. By this time, too, Dr. Pearson's [5] book had come to his hand. This amply supported Jenner's views on the importance of vaccination and is the second significant article in the English language on vaccination. Dr. Waterhouse confesses that he was forced to recapitulate from memory all that he could tell the Academicians of Pearson's views because, apparently, in his enthusiasm to promote the cause of vaccination he had lent his own copy of Pearson's book to one of his friends, and like so many books that are loaned, it had departed forever from his library.

The members of the Academy of Arts and Sciences appeared dubiously interested in the matter. On the one hand, Dr. Waterhouse says that the reception to his communication was entirely satisfactory and that it provoked so much discussion and questioning as to make him promise to prepare a memoir on the topic of vaccination for a future meeting. On the other hand, the minutes of the Academy make no mention of any remarks by Dr. Waterhouse on the subject of vaccination during that season. Perhaps he spoke privately of this new procedure to the President [6] of the Academy, and to a few other members who seemed so much interested as to encourage him to go ahead. In any event, before he had time to prepare any paper, Woodville's [7] book arrived in Cambridge—the third pioneer article in the field of English vaccination literature. Waterhouse modestly said that this was so perfect a piece of work that there was no sense

[5] Pearson, G., An inquiry concerning the history of the cowpox, 1798.

[6] Lest this should ever be read beyond the bounds of New England, it will not be superfluous to add here that the President of the American Academy of Arts and Sciences was Mr. John Adams, President of the United States.

[7] Woodville, W., Reports of a series of inoculations for the variolae vaccinae, or cow-pox, 1799.

in his trying to write anything better for the Academy, that it contained a chain of facts in favor of vaccination stronger than any that he could possibly assemble, and also—and this lent weight—that

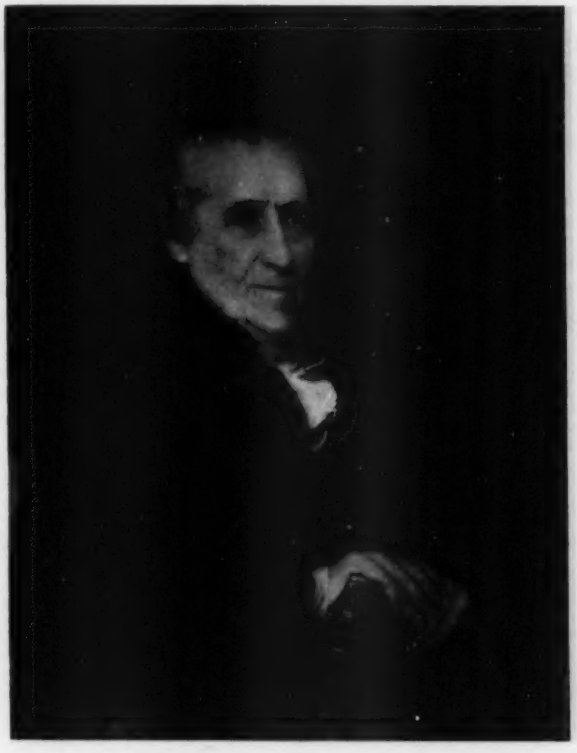


Fig. 2.

BENJAMIN WATERHOUSE.

(Portrait by Frothingham in Harvard Medical School Library. The book which the Doctor holds is entitled, "Jenner").

everything that Woodville said was backed up by his old friend Dr. Haygarth of Bath whom Waterhouse regarded as the one man then alive most thoroughly acquainted with the laws of contagious diseases.

Dr. Waterhouse by now was convinced that vaccination was of fundamental importance but still he moved carefully. He wrote to a number of his friends in England, asking that new articles dealing with vaccination be forwarded to him as quickly as possible, and he took pains to obtain opinions from those of his British friends whom he knew to be most conservative, from men, he said, who always were in the habit of objecting much to new ideas and of acting upon them slowly.

So at last it happened that in the early spring of 1800 Dr. Waterhouse was as well read as anyone in America on the subject of vaccination. He knew Jenner's original article almost by heart and was thoroughly familiar with what was going on in England regarding the new method. His mind was made up; hoping to effect a public benefit and conceiving it a duty to do so if he could in his official situation in Harvard University, he determined to attempt the *experimentum crucis.*

To obtain potent vaccine virus in Boston was no easy task. No one in New England professed to know anything about the cow-pox so that the disease had to be introduced there, and how best to do this was largely a matter of conjecture. Various suggestions were offered: perhaps the most original was that of vaccinating a suitable sailor just before a ship sailed for Boston from a British port, and when his arm grew sore, of transmitting the cow-pox from him to another non-immune sailor and so on all the way across the sea. The proponents of this idea said it had a double advantage: there would arrive in Boston a man with a nicely ripened case of cow-pox who could be used as a donor for the New World, and a lot of other sailors who landed there would henceforward be protected from acquiring the smallpox—which would be a great blessing to all of them.

Knowledge about cow-pox in the meantime was developing steadily in England. That the virus in many ways was tough and difficult to kill yet at the same time fragile and easy to lose, had already become apparent. Even Dr. Jenner himself on occasion had lost his virus strain and had been forced to borrow a new planting from one of his friends. This experience led him to formulate what

he termed the golden rule in vaccine inoculation:[8] never to use the virus from a donor after the formation of an efflorescence around the pustule or when the fluid of the pustule had lost its limpid quality.

It became apparent, too, that vaccine virus taken on a lancet soon corroded the metal and was quickly decomposed. On the other hand, various methods were being successfully adopted for conveying the vaccine fluid to distant parts of Europe in a state of activity; a piece of cotton thread might be well moistened with fluid obtained from a recent vaccination blister, allowed to dry in the open air, and not by the heat of a fire—for the virus was not particularly heat resistant—and then rolled up in a scroll of writing paper or put in an envelope or enclosed in a glass bottle. Or the matter might be kept between flat glasses sealed with wax or gold-beater's skin, eventually to be taken up in a drop of water and used several weeks later with complete success. All these facts led Dr. Jenner to surmise that viable material might easily be sent his transatlantic coadjutor, as he termed Dr. Waterhouse, and he was fully as eager as Waterhouse to see the experiment tried.

The exact deliberations that occurred in England concerning the project are not known. Jenner and Lettsom undoubtedly had a hand in them, among others, along with Dr. Haygarth of Bath and a young surgeon there named Thomas Creaser who was an early vaccinator, having been taught the tricks of the trade by Jenner himself. The general idea seems to have been to have sent to Dr. Waterhouse by the quickest possible route some properly prepared material. With this thought in mind, young Creaser in Bath was directed to prepare some threads frestly coated with virus, to place them in a tightly stoppered glass bottle, to put them aboard a Boston-bound boat just before she sailed from Bristol, and to instruct whomsoever was made custodian of this precious bottle to deliver it to Dr. Waterhouse as quickly as possible upon arrival. Presumably Creaser was selected because of his known technical ability to prepare the vaccine virus properly, and Bristol was chosen as the port of departure because a package placed on a ship there would have quicker sea passage to

[8] I am told by modern vaccinators that when the efflorescence forms and the pustule fluid becomes cloudy, enough antibodies are present so as to make a "take" from such fluid unlikely.

Boston than would a package shipped from London and thus have less time in which to disintegrate from the rigors of a long ocean voyage.

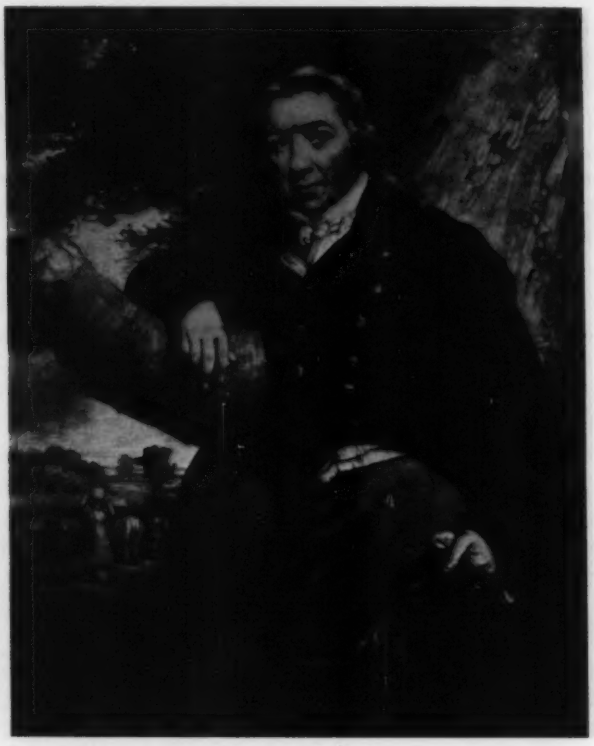


Fig. 3.
EDWARD JENNER.

The " Foxwell " left Bristol on the 3rd of May 1800, arriving in Boston on July 4th. As she came to anchor off Long Wharf she fired a grand federal salute, perhaps thus celebrating the arrival of the first vaccine virus to be brought to New England. The " Foxwell " was the only ship from Bristol to arrive in Boston around that time hence it is not unreasonable to credit her with carrying along with

a cargo of paving stones, warming pans, pots and kettles, nails, some gun powder, a quantity of fine blown salt, and several barrels of Bristol Crown Glass, the precious bottle containing Dr. Waterhouse's threads impregnated with kine-pock virus. He hardly would have kept the material around the house for long without using it, and on July 8th 1800, he performed his first vaccination. Exactly from whose hands he received the bottle is not known: possibly from Captain Henry Barnard's, of the "Foxwell" or from the hands of her solitary first class passenger, a Mr. Mardenbrough.

Suffice it to say that like Zabdiel Boylston before him, he began experimenting with his own family:[9] "The first of my children that I inoculated, was a boy of five years old, named Daniel Oliver Waterhouse. I made a slight incision in the usual place for inoculation in the arm, inserted a small portion of the infected thread, and covered it with a sticking-plaster. It exhibited no other appearance than what would have arisen from any other extraneous substance, until the 6th day, when an encreased redness called forth my attention. On the 8th, he complained of pain under the inoculated arm, and on the 9th, the inoculated part exhibited evident signs of virulency. The sore in the arm proceeded exactly as Dr. Jenner and Woodville describe, and appeared to the eye very like the second plate in Dr. Jenner's elegant publication. In short, the appearance and symptoms of this disease, in the *old* world, and in the *new*, were more completely alike than I expected."

A few days later he inoculated a younger son, Benjamin, Jr., with material from Daniel's arm, and from him a baby daughter, Mary, and her nursery maid. He inoculated also a servant boy with some of the infected thread from England, and, before long, a daughter Elizabeth and two other domestics. They all had good "takes" and were Waterhouse's first series of cases.

He was not satisfied with this result for next he felt it necessary to find out whether the persons so vaccinated in America were really immune to smallpox as in accordance with Dr. Jenner's experience in England. So he sent young Daniel to Dr. Aspinwall's smallpox

[9] This and the following quotations from Dr. Waterhouse are taken from his publications, from an autobiographical manuscript in his own handwriting, or from letters now in the Harvard Medical School Library.

hospital in Brookline and there watched Dr. Aspinwall inoculate him with matter taken that moment from a patient who had the inoculated smallpox " pretty full upon him," and to make doubly sure with an infected thread. Finally, Daniel was placed in bed alongside of a patient ill with ordinary smallpox. On the 4th day Daniel's arm grew sore, but in a day or two it dried off, and grew well, without producing the slightest trace of any disease. Daniel did not develop smallpox and was dismissed from the hospital on the 12th day after the experiment began none the worse from being such a Spartan guinea-pig. Dr. Waterhouse, in no way inflated with parental pride, merely remarked a trifle complacently that one such observation was worth a thousand arguments. Dr. Aspinwall was less impersonal. Instead of stating that this was no humbug, as Bostonians are expected to do when they observe something new and curious in the medical line, he said almost the same thing in slightly different words, perhaps a little wistfully, " This is no deception. I rejoice at the discovery as a friend of humanity although it must deprive me of a very handsome annual income."

In this fashion was vaccination introduced into America.

Dr. Waterhouse presently found himself tempted to build up a new and lucrative medical business as the result of his experiments. The news spread quickly. By the first of September, to quote Dr. John Warren, the kine-pock was making good deal of noise around Boston. A little later Dr. Waterhouse was assailed by practitioners all over New England to supply them with vaccine virus—at very generous terms—in order that they might take advantage of what promised to become a veritable medical bonanza. He considered this not the correct way to proceed, and yet in spite of all that he could do there developed in no time at all " a singular traffic in the article of vaccine matter by persons not in the least connected with the profession as stage-drivers and peddlars. Vagrant quacks strolled about the country inoculating for half a dollar a head and several for a quarter that sum; this impudence was not confined to such characters; several young practitioners pursued the same disgraceful line." His struggles against that sort of thing and his heroic fight to establish vaccination on a proper basis in this country have been described by many pens more talented than mine. There are innumer-

able stories connected with it, one that I like particularly being his own version of the energetic[10] manner in which the situation in Rhode Island once was handled.[11] "They never did anything in vaccination when the smallpox was brought into Newport. They inoculated a few, but were much inclined to inoculate with *small pox*; and perhaps would have committed this mad and wicked act, had not my son Andrew, who resides there, prevented it by the strongest of arguments. He told them that should his father hear of it, he would come himself to stop it; and if he could not, would publish them to the world as fools or knaves or both. He told the men in authority there, that it would be a double disgrace to Newport, where the first promoter of the Kine Pock inoculation was born, to encourage such an unwise project. He soon gained men of the most influence on his side; and they arrested the contagion before it had done any very serious mischief, or allowed of such a disgraceful precedent."

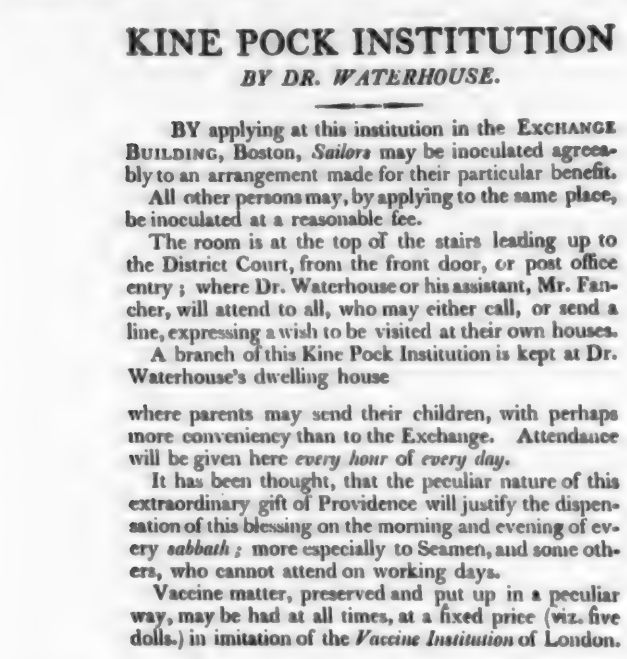
One of the pleasant and important by-products of the vaccination struggle was the staunch friendship which grew up between Waterhouse and Jenner. Although actually they never saw one another, Dr. Waterhouse came to feel that Jenner to him was what the sun is to the moon, and Dr. Jenner to look on Waterhouse as an interesting and original character unlike any other of his immediate circle of acquaintanceship and a man of whom he grew very fond. Letters passed between the two for several years: the few that are known are interesting for the informal medical information they contain and for the manner in which they are written. They are models of urbanity making any reader agree with Sam Weller that the great art of letter writing is to make the recipient wish there were more.

This friendship began entirely by accident. In the fall of 1800 Dr. Waterhouse found himself in trouble: "This autumn a serious

[10] From a letter to his son John while a student at Andover Academy, "I love to see *energy* in everything even in quarreling. Alexander conquered the world by *energy*. Caesar attained the height of power and glory by *energy*. Bonaparte holds the reins of Europe by *energy*. Nothing can be attained worth acceptance without *energy*; everything within the circumference of human power can be acquired by *energy*. It is the *will*; the *desire* that raises one man above another. It is not good luck but *energy*." (Harvard Medical School Library.)

[11] From a letter to Dr. Sylvanus Fansher, June 8th, 1827. (Harvard Medical School Library.)

occurrence at Marblehead attracted the attention of the public; a practitioner inoculated his children with smallpox matter, which he supposed to be the vaccine matter. This spread the smallpox through that seaport, and forty persons who had been inoculated with spurious



KINE POCK INSTITUTION
BY DR. WATERHOUSE.

BY applying at this institution in the EXCHANGE BUILDING, Boston, *Sailors* may be inoculated agreeably to an arrangement made for their particular benefit.

All other persons may, by applying to the same place, be inoculated at a reasonable fee.

The room is at the top of the stairs leading up to the District Court, from the front door, or post office entry; where Dr. Waterhouse or his assistant, Mr. Fancher, will attend to all, who may either call, or send a line, expressing a wish to be visited at their own houses.

A branch of this Kine Pock Institution is kept at Dr. Waterhouse's dwelling house

where parents may send their children, with perhaps more conveniency than to the Exchange. Attendance will be given here *every hour* of *every day*.

It has been thought, that the peculiar nature of this extraordinary gift of Providence will justify the dispensation of this blessing on the morning and evening of every *sabbath*; more especially to Seamen, and some others, who cannot attend on working days.

Vaccine matter, preserved and put up in a peculiar way, may be had at all times, at a fixed price (viz. five dolls.) in imitation of the *Vaccine Institution* of London.

Fig. 4.

Broadside of the Kine Pock Institution of Boston, a medical venture which failed.

kine-pock matter took the distemper, and a sudden downfall of the credit of Vaccination followed, together with not a few execrations on the original promoter of it." Naturally, Dr. Waterhouse was much disturbed, particularly when so sage an adviser as President Willard of Harvard University got wind of the matter and told him that he had collected the opinions of reliable doctors in Boston, Salem, and Beverly and had honestly come to believe that so much trouble had been stirred up by this outbreak in Marblehead and so much

doubt had been cast on the usefulness of vaccination as to make it appear to him inexpedient for a Professor in the college to be mixed up in the business.

Dr. Waterhouse, however, was a stubborn soul. Moreover, there was one thing about the Marblehead outbreak which attracted his attention and gave him faith: those townspeople whom he himself had vaccinated escaped the smallpox although many of them slept with people ill with the disease.

He was in an awkward predicament and therefore moved cannily. "By the end of Nov^{r} spurious cases multiplied so fast upon me and everyone else that I was glad to wind up the business with the year. I therefore gave out that the winter would not be so favorable to the inoculation as the spring; and at this perplexed stage of the business I wrote to my correspondents in Eng^{d}."

First he wrote to Dr. Lettsom describing what had occurred, believing it in part due to deterioration of the virus, and suggesting that it would be nice if Dr. Jenner could be consulted. Thus on March 4th, 1801,[18] Dr. Jenner wrote his first letter to Waterhouse. It is a long letter, including some general rules regarding vaccination, a fugitive illustration from his next pamphlet, and a great deal of good advice. Parts of it are quoted because they show how sympathetic Jenner was to a struggling clinical investigator in far distant land and how glad to lend a helping hand.

"Previously to the transmission of your letter to me, by Dr. Lettsom, I had heard of the sad embarrassment you had fallen into from the loss of your vaccine matter, and most ardently do I hope that the efforts I have used with the view of your being resupplied, may long before this time have proved successful. . . .

"By the conveyance which brings you this, you will not only receive vaccine matter, such as I employ with my patients here, and among them I frequently find the offspring of some of the first families in our Realms, but such laws also for conducting the vaccine process, or if you please, such a map of the road you are to pursue, as will in future prevent forever your losing the way.

"I am about to publish a fourth Tract upon the cow pox, and am sorry that it is not yet in print, or with pleasure would I send it to you. . . . But although I am thus precluded from sending you the pamphlet, that infor-

[18] From Waterhouse's, A Prospect of exterminating the Small Pox. Part II, 110.

mation which may prove most satisfactory to you will lie in a small compass, and shall form the principal subject of this epistle.

"The vaccine fluid must be considered as especially delicate in its texture. . . . The perfect virus only can produce the perfect vaccine pustule, at least the fluid inserted must contain some particles of it in its perfect state.

"Now I conceive that at some period of your inoculation, which may now have escaped your recollection, an imperfect pustule arose, either from some peculiarity in the constitution of your patient, or some alteration in the qualities of your matter, and that from this stock you propagated. The consequence was, that continued degeneracy you complain of in the nature of the disease. . . .

"I am aware that it is the opinion of some, that the vaccine matter loses its properties after it has passed from the cow through a given number of human subjects. . . . I am now, almost daily, inoculating children in the metropolis; and producing with it the vaccine disease with all its characters. . . . In short there does not appear to be the least tendency toward change in the nature of the virus *from time*. . . .

"It is unnecessary for me to say that the vaccine virus I now convey to you is perfectly genuine, when you may be assured it is from that stock which I am using among all my patients here, and these are of every order from the Peer to the Porter; for all ranks of society readily embrace our new doctrine. . . .

"This letter ought certainly to be written over again, but Dr. Lettsom has just sent a message that forbids it. It is of no less magnitude than that the ship, which is to convey it across the Atlantic, is about to sail.

"It will afford me much pleasure and satisfaction to be favoured with your correspondence on this subject, or any others. Sincerely wishing you health and happiness, and complete success with your inoculation, I remain,

Dear Sir,
your obliged and very faithful
humble servant,
Edward Jenner.

"P. S. Some of the virus I have sent was taken from a pustule this morning by my friend Mr. Ring,[18] *Surgeon in New Street Hanover Square*. We

[18] John Ring who wrote "A Treatise on the Cow-Pox," the first volume of which was published in 1801. Of vaccination he says on page 443, "It has crossed the waves of the wide Atlantic, and been adopted by Dr. Waterhouse, the learned Professor of the Theory and Practice of Physic in the University of Cambridge. This university is situated about three miles from Boston. Here the bright beam that was to illume the realms of Columbia, first dawned. Here the Jenner of America first appeared;

And a new sun in the new world arose.

occasionally assist each other with the fresh fluid. The whole is from my original stock. I have sent you one of the coloured plates, which will be published with my next pamphlet, to shew the progress of the perfect pustule.

E. J."

Dr. Waterhouse was both delighted and flattered when he received this. On April 22nd, 1801,[14] he wrote Dr. Lettsom:

"I received your parcel, containing the two publications on the cow-pox, and also a packet from Dr. Jenner with *fresh* matter, and a lengthy and most admirable letter. It was, like his publications, plain and lucid beyond his compeers; displaying that perspicuity and dignified simplicity which is the peculiar ornament of genius. His letter is (excuse my enthusiasm) a ray of light from the East, and oh! that the reflected ray might become more brilliant; and even generative at the point of repercussion! I shall write to this *primitive apostle* by the Galen; should you in the mean time see him, make my most cordial acknowledgment to him: tell him he receives not only my thanks, but the thanks of the American public; for the name of Jenner is familiar in every village in New England. . . .

"I should be grateful to know more of Jenner's [15] personal history. Is he not a son of Oxford, or Cambridge? I have never heard any particulars concerning him; but from his logic, I suspect he is a son of one of them, or else he is like Franklin, one of Nature's own pupils. Some men are destined to follow the rules of colleges, but with others, *rules follow them.*"

His letter to Jenner written on April 24th [16] is even more ebullient:

"Being just informed of a ship's sailing tomorrow for London I have only time to acknowledge the receipt of your most excellent letter. . . . With it I received a supply of vaccine matter, which came to hand thirty-eight days after the date of your letter, for which you have my most cordial thanks. I have inoculated with it, and found it good, and here send you the first crop from it. When my good friend Dr. Lettsom [17] has sent me curious melon

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[14] From Pettigrew's, Memoirs of the Life and Writings of the late John Coakley Lettsom. Vol. III, 212.

[15] This was for a short biographical sketch which Waterhouse published later.

[16] From Baron's, The Life of Edward Jenner, M. D. Vol. I, 439.

[17] Dr. Waterhouse and Dr. Lettsom apparently reciprocated in the interchange of seeds as well as medical opinions. On March 9, 1801, Dr. Waterhouse sent Dr. Lettsom some pumpkin seeds which he found on the Island of Nantucket. He was told that whale-men had brought the original seeds from Cape Horn, and hence they were called locally, when they grew, Cape Horn Pumpkins. Dr. Waterhouse told Dr. Lettsom that to cultivate them properly, they should be planted like cucumbers and given plenty of manure; they made admirable puddings or bread, "when mixed with half flour or Indian meal." In September 1840, Dr. Waterhouse claimed to have one growing in his garden "almost as big round as a beer barrel." What luck Dr. Lettsom had with the seeds is not known.

seeds I have sent him as soon as possible some seed raised from them, that he might see whether our soil, atmosphere, and mode of culture effected any alteration from the original stock. The same I have now done with your Vaccine virus. . . . I took the patient into my own house that I might watch the progress of the local affection, which I did under the microscope. It is now the tenth day in the morning, and I expect the efflorescence will, in ten or twelve hours more, put on the appearance of your *tenth day representation* in the coloured engravings.

"It is impossible for me to express the great satisfaction your letter gave me. The subject was before involved in a mist: your letter was a ray of light, which ray must be reflected for the benefit of the western world. Oh! that it were possible for this ray to become still more brilliant and even generative at the point of repercussion.

"I entirely agree with you as to the cause of our late failure in inoculation. . . . You know not what it is to be perplexed in this business. That prince of physiologists, John Hunter, once told me that 'he loved to be puzzled, for then he was sure he should learn something valuable.' Burthensome as it was at the time I do not now regret my perplexity. When I had lost my way, and wandered into the wilds of conjecture, I stood still. . . . Now we[18] are going on again, but not with the faith and spirit of the last season. . . .

"Accept my thanks for the coloured plate. It is indeed a happy expedient, and honours the graphic art. It is thought here to be so important that I am anxious to know if I can with propriety procure some more of them. I should wish to possess a couple of dozen to be deposited in the hands of some of our leading practitioners, or clergymen, in different parts of the United States by way of standards. . . .

"I have been informed from a quarter not likely to be deceived, that cows[19] (contrary to my assertion in page 22 of my pamphlet) have been known to have the small-pox.

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[18] Presumably this does not refer to Waterhouse himself but rather to New England physicians.

[19] Dr. Waterhouse puzzled over the relation between cow-pox and smallpox for many years. In 1827 he wrote Dr. Fansher: "I have believed from the beginning of my knowledge of vaccination that such an epizootic distemper was amongst our kine. I grounded my belief upon the origin of our cows, & the similarity of the country of *New* England with that of the old. . . . I have had three or four very strong *presumptive* evidences of milkers having taken the distemper from milking, one given me by Dr. Willard of Vermont, and another, still stronger, by a Mr. Nathan Adams now living in Charlestown, near Boston; but I never had that plenar of evidence which would satisfy a Judge in court of law on a trial of life & death. But the case you mention is of a nature to determine the question beyond all shadow of doubt, if you have seen the eruptions on the udders of the cows & the pustules on the hands of the milkers, and above all if *you have given the true disease to one of the human species.* And I am in hopes to hear from you that you have been confirmed in your opinion by *inequivocal experiment.*

"The account is this. At one of our periodical inoculations, which occurs in New England once in eight or nine years, several persons drove their cows to an hospital near a populous village, in order their families might have the daily benefit of their milk. These cows were milked by persons in all stages of the small-pox: the consequence was, the cows had an eruptive disorder on their teats and udders, so like the small-pox pustule, that every one in the hospital, as well as the physician who told me, declared the cows had the small-pox. Since the cow-pox has been talked of this account has been revived and credited. Have you found any thing like this in England?

"I inoculated one of my cows with the Vaccine virus, and obtained from her a crop of matter on the ninth day, which produced the disease in the human subjects to perfection. Is this experiment known among you? As I operated myself there was no avenue opened for deception in the whole experiment. . . .

"You very politely express a wish for more of my letters on the Vaccine or any other subject. In order to damp this desire and surfeit you at once, I have directed my book-seller to send you a whole volume of them, which the partiality of Dr. Lettsom has brought into light. . . .

"I need not, I think, say how highly I should prize the correspondence of Dr. Jenner on any subject; but more especially on that for which he is so deservedly celebrated; and who, according to my understanding, is the only *clear, consistent,* UNCONFUSED writer on the cow-pox that has yet appeared.

"I reiterate my thanks for your kindness, and beg you to accept the assurances of high consideration and esteem!

"P. S. As the library of this University is by far the largest in the United States, and is the grand deposit of rare and valuable books in this quarter of the world and will continue so, I cannot resist expressing my wish that a copy of your invaluable work may be deposited there by its author. I presume my motives for wishing this, and hinting it, stand in no need of an apology. By a law of the Commonwealth, an author to secure his copy-right must deposit a copy of his work in this library; and books sent to it come free from duty. This library, museum, and other public rooms are constantly visited by strangers as among the curiosities of the country. When I had the honour of waiting on the Duke of Kent through them, he expressed his surprise at such a collection of books and material productions in about thirty years, *for the small-pox destroyed the chief of what had been collected since* 1638: that is to say, it raged in Boston, and the legislature on that account occupied one of the public rooms in the hall, which contained the library; when it by some accident took fire and was, one alcove excepted, totally destroyed. Thanks be to Dr. Jenner, such an accident from *such a cause* can never happen again."

On the 30th [20] of May he wrote a follow-up letter to this, of chief

[20] From Baron's, The Life of Edward Jenner, M. D. Vol. I, 469.

interest as it so well illustrates his characteristic energy and some of his perplexities:

"I now send you a portion of your own virus, being a part of that *identical* thread which you, in conjunction with Mr. Ring, took from the arm of a patient on the *third day of last March* in London, that experiment may be

Fig. 5.

Cow Pox Virus in a fine female arm of about two-and-twenty years' standing. From a water-color painting found opposite the title-page of Waterhouse's own copy of Jenner's, "The Origin of the Vaccine Inoculation." 1801.

made respecting its activity at a still greater length of time. . . . It is now within four days of being *three months* old, and it had not lost its activity ten days ago. . . .

"You will doubtless rejoice with me that the vaccine inoculation is progressing here to my entire satisfaction. I began with the matter you sent me the 24th [21] of March, and have inoculated not quite a hundred, and have not had one dubious case among them all. I have given the virus to most of the

[21] Obviously a misprint. Jenner's letter was written on March 4th.

leading physicians in Boston and its vicinity. But, alas! poor human nature! thou art the same in *New*-England as in *Old*." . . .

What Waterhouse referred to in the last sentence is better explained in his own words. He did not like the Massachusetts Medical Society nor did its members like him. "Early in the spring of 1801, I was met by Dr. Isaac Rand in one of the streets of Boston, who told me that the Mass. Med. Society had just imported some vaccine matter from London; and that they had distributed it among two or three of their members, and that it had failed in their hands; and that he was fearful that what he himself had used would fail also; but of this he was not certain, he spoke as if he had as much to fear as to hope on the matter. On hearing this, without saying a word of my intention to Dr. Rand, I at once resolved to send a fresh supply of matter to the Medical Society, with a view to prevent their disappointment. I therefore on returning home, put up with great care vaccine matter of various ages, both recent matter, as well as some I had just received from England, and the next day I sent it to the Medical Society. . . . These gentlemen did not chuse to be under obligation to me for matter, nor to notice the offer I made them." Once he remarked that the natives of America were skilful in bush-fighting and that had he not a kind of apostolic zeal he would at times feel a little discouraged!

On the whole, however, by virtue of the shipment of vaccine virus described above, Waterhouse now found himself able to carry on successfully. By following Jenner's rules he went ahead to spread the gospel of vaccination, not, however, without meeting other difficulties than an unfriendly attitude of the Massachusetts Medical Society. For example, in 1802, for a time, he found himself obliged to hire volunteers to be inoculated in Cambridge in order that he might maintain a certain continuous supply of fresh virus.

There is a very pleasant letter from Jenner to Waterhouse written on February 24th,[22] 1802.

[22] In Dr. Waterhouse's pamphlet of "Information respecting the origin, progress and efficacy of the Kine Pock Inoculation" published in 1810, there is a curious typographical error. Dr. Jenner's letter is quoted in full, beginning on page 49. It is dated "London, Bond Street, February 24, 1801." Actually it was written a year later. Dr. Jenner's extreme busyness at the time perhaps explains this slip of the pen.

"I believe, according to the laws of true politeness, I should begin this letter with an apology. Allow me then to request you to pardon my seeming inattention. I say *seeming*, for be assured you are frequently in my thoughts; and when I think of you, I foolishly long for powers that a mortal ought not to aspire to. Nothing less than a trumpet, not that *whispering* thing used by the mariner, but one that would carry my voice on the rapid wings of the wind across the wide ocean that divides us. . . .

"What a lamentable thing is distance? distance almost immeasurable between friend and friend!—Would I could say, will you dine with me? or take your coffee with me tomorrow, that we may talk over fully our vaccine affairs? But alas! so great is the space between us, you might as well receive my invitation in the moon, nay the georium sidus, as on the other side of the Atlantic. We must do the best we can under these disadvantages, and be thankful that the ingenuity of man has devised ships, and given speech to *feathers*.

"Of the vaccine subject I fear I shall have little new to offer you. Knowing that you are in possession of the writings of Mr. *Ring* upon the vaccine inoculation, I shall find it difficult to give you any thing worthy of observation. . . . His work will contain a fund of information, both for present times, and those which are to come. . . .

"The most arduous task that I have lately had to execute has been that of keeping practitioners in order; making them sensible of the absolute necessity of attending to the *quality* of the virus employed. You will receive with this some rules, compressed in a small space. I have sent you also some virus from a new stock, the history of which you shall hear. . . . A medical gentleman at Milan, Dr. *Sacco*, who informed me he has inoculated 8000 persons in that city, has lately sent me vaccine virus, taken from a dairy on the plains of Lombardy. It has produced again and again the perfect pustule here. I always ventured to predict, that the cow pox was not confined to this island only. . . .

"And now, my good Doctor, I would fain proceed further, and *settle my epistolary account* with you; but our friend *Spalding*[28] tells me that if I do not make haste, the ship, intended to convey this, will be gone. Spare then *my life* a little longer.—I have not said half I wish to say, nor taken that

[28] Matthias Spalding, a graduate of Harvard College in 1798 and a protégé of Waterhouse, to whom Jenner took a great fancy. When he went to England in the fall of 1801, Waterhouse gave him proper letters of introduction and presently wrote, "I was grateful at hearing of the polite attention of Dr. Lettsom to you. . . . I was pleased to hear that you had conversed with the man I so much admire, I mean Jenner. Present my best regards to him and tell him that although I sought his life with eagerness, and failed in the attempt, I have nevertheless (instigated by the same spirit) been a little consoled in *hanging him up, in effigy*, in my parlour."

notice of your excellent epistles so fully as I ought. But I am this moment fifty letters behind hand with my correspondents—a distressing idea."

Dr. Waterhouse lost no time in answering this for on April 8th,[24] he replied:

"Four hours ago I received your polite and very interesting letter dated 24th. February. The ship had but thirty-two days passage. I have just put your Lombardy virus into a fine female arm of about two-and-twenty years' standing, and shall give you the result by the return of the ship which brought it. . . .

"Dr. Rush has come out full and strong in praise of the new inoculation and has sent me a copy of an eloquent lecture of his on the blessings of the Jennerian discovery. . . .

"Last December a grand embassy of certain tribes of the Indians came to the city of Washington while the Congress was sitting. . . . The chief of this embassy was named *Little Turtle*. The President one day sent for the warrior and his interpreter. . . . He then told him that the Great Spirit had lately made a precious donation to the enlightened white men over the great water, first to a single person, and from him to another on this side the waters, and then explained to him the history of the cow or kine-pock as a gift from Heaven to preserve them from the small-pox, and even to banish it from the earth. The chief heard him with marked attention, and desired first to receive the benefits of it himself. . . .

"When the Minerva returns I may possibly write you a *letter* instead of this *receipt* for your valuable favours; as it is you will receive the cordial wishes of an affectionate friend."

The year of 1802, was not without interest to the friendship between Waterhouse and Jenner in other ways than by correspondence. It was in the autumn of this year that Waterhouse added the finishing touches to his book on Vaccination, the preparation of which he said represented the labor of more than six months and an investment of more than 50 guineas. He felt bitterly over the way in which it was received: there were scarcely more than half a dozen copies bought by Boston physicians and the critics [25] of the Massachusetts Medical Society humiliated him by terming his book "obscure"

[24] From Baron's, The Life of Edward Jenner, M. D. Vol. I, 593.

[25] In 1808, the Massachusetts Medical Society appointed a committee to report on vaccination. The committee rendered a lengthy report in which Waterhouse's name does not appear nor is any reference made to his efforts towards introducing vaccination in America.

To JOHN COAKLEY LETTSOM;

AND

To EDWARD JENNER;

PHYSICIANS PREEMINENTLY DISTINGUISHED FOR

THEIR ACTIVE BENEVOLENCE

AND

PROFESSIONAL SKILL,

THIS ESSAY

IS INSCRIBED,

AS A MARK OF ESTEEM AND RESPECT,

BY THEIR TRANSATLANTIC FRIEND

BENJAMIN WATERHOUSE.

CAMBRIDGE *New England*,
November 1801.

Fig. 6.

Dedication of the book by Dr. Waterhouse, "A Prospect of exterminating the Small Pox." 1802.

and not worth looking at. He would have been badly hurt had not his British friends helped out and had not the volume sold fairly well in the South.

To compensate for this, he received an unusual present from England in that same autumn. His letter[26] to Dr. Lettsom of November 16, 1802, describes this nicely:

"I have sent, by this opportunity, a few pages of my publication on the Kine-pock, to Mr. Ring. The whole will be printed within a month. Every body will approve of the absolute propriety of addressing it to yourself and to Dr. Jenner, when they know that the first intimation I ever had of the cow-pox came from Dr. Lettsom, and that Dr. Jenner has been to me what the *sun* is to the *moon*. Should any of you think this book of sufficient importance to admit of a London edition, I shall add to it an Epistle Dedicatory, expressive of these obligations. But more of this hereafter. I shall bear in mind the LL. D. for Dr. Jenner next July. We give degrees but once a year. Dr. Jenner has just sent me a present I highly prize, a silver box inlaid with gold of exquisite taste and workmanship, bearing the inscription "Edward Jenner to Benjamin Waterhouse." But Mr. Ring annexed the superscription in rather an hyperbolic style, "From the Jenner of the Old World to the Jenner of the New World." Long will it remain among the sacrae relictae of my family."

The silver box is fully as attractive as Dr. Waterhouse described it and now is in the Harvard Medical School Library, a living token of affection between two unusual friends.

At Commencement in 1803, Dr. Jenner was duly made an Honorary alumnus of Harvard University with the degree of Doctor of Laws, and Waterhouse had the pleasure of sending the diploma to his friend in England. The citation,[27] which seems to have a Waterhouse touch to it, is as follows:

"A profound scholar of medicine, anatomy, and physiology, endowed with discerning originality and pre-eminent now because of his brilliant investigations on inoculation with cow-pox through which smallpox, a disease destructive to the human race, may be outrooted; a wise clinician, master, too, of other arts and sciences which add beauty and usefulness to human endeavor."

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[26] From Pettigrew's, Memoirs of the Life and Writings of the late John Coakley Lettsom, Vol. II, 481.

[27] From the Harvard Archives. The original citation was in Latin. This translation has the sanction of Professor E. K. Rand.

After this year letters between the two are more difficult to uncover. In 1807, when Dr. Waterhouse appealed to the Massachusetts legislature for financial compensation because of his vaccination

Fig. 7.

The Silver Box: "From the Jenner of the Old World to the Jenner of the New World." 1802.

efforts, Jenner [28] wrote rather perfunctorily that "the granting of favors to the man whom I have the satisfaction to rank among the earliest, the most active, and the most successful of my transmarine disciples, I shall ever feel as an honor conferred upon myself." Jenner

[28] From Waterhouse's, Information respecting the origin, progress and efficacy of the Kine Pock Inoculation, 48.

seemingly wrote this for publication, or so that Waterhouse might use it as he saw fit and hence it lacks all personal warmth.

By 1810, with the threat of war in the offing, all friendly correspondence between England and New England seems to have dwindled. Waterhouse wrote Lettsom [80] in May "what a tedious length of time has elapsed since I had a line from you, or indeed, any of my old friends and correspondents in England! . . . Where is Jenner? and where is Ring? There is, I hope, no embargo on their friendship." In December [80] of the same year he wrote, "I believe it is near upon two years since I have received a line from Dr. Lettsom; and more than a year since I had one from Dr. Jenner. Has our Act of Congress occasioned this *non*-intercourse between old friends?" On the other hand, some degree of friendship between Jenner and Waterhous managed to survive the ordeals of war for in 1812, Jenner [81] mentions having received recently some political news from Waterhouse, ". . . a man of correct habits. For the first seven years of vaccination I corresponded with him regularly. He upbraids me justly for late irregularities." And in 1817, Mr. John Adams [82] wrote to Waterhouse, "The Calamity of your friend Jenner affects me very Sensibly." From which one may assume that the Cambridge physician knew of the illness which prostrated his friend six years before his death.

The Metropolitan [83] Life Insurance Company has published recently an interesting account of the decreasing incidence of smallpox in the United States. The writer of this article concludes with the statement that the disease is now a public health anachronism. Methods for its control are well known and measures for its prevention are at hand: in the near future it should be completely wiped off the public health slate of this country.

The medical knowledge on which such a statement depends originated in England more than a hundred and forty years ago by virtue

[80] From Pettigrew's, Memoirs of the Life and Writings of the late John Coakley Lettsom. Vol. II, 484.

[80] From *Ibid.* Vol. II, 491.

[81] From Baron's, The Life of Edward Jenner, M. D. Vol. II, 382.

[82] From W. C. Ford's, Statesman and Friend, 134.

[83] Statistical Bulletin, Metropolitan Life Insurance Company, 22: 6 (June) 1941.

of the accurate observations and deductions of a keen clinician. It was transmitted to New England and spread through the New World largely by the persevering efforts of a well-read and fearless clinical investigator on this side of the ocean. Friendship, in one way or another, had a good deal to do with making this possible. That friendship between two doctors who never met should have played an important part in the achievement of a result so vital to our national health as the prevention of smallpox is, indeed, something curious in the medical line.

HARVEY'S APPRAISAL OF HIS *DE GENERATIONE*

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It has often been said that Harvey would not have published his work on generation but for the entreaties of his bosom friend, Sir George Ent, sometimes referred to derogatively in this connection as " old Ent." That such a conclusion is untenable for other reasons than some presented here, was indicated elsewhere (Meyer, 1936).

It long has seemed improbable to me that none of the many competent authorities who have discussed Harvey's views on the circulation and the *De Motu Cordis* have commented upon the startling words to which I shall direct attention, but I know of no one who has done so. I am compelled to say the same thing regarding the few who have devoted more or less attention to his *De Generatione Animalium*. This silence may be due, in part, to the fact that the words are contained in neither of these works but in one of his letters. This does not imply, however, that they therefore were ill-considered. They are found in the second of Harvey's letters to Riolan, in which the former continued a critical rebuttal of his ideas on the circulation, some of which were controverted by the renowned French anatomist. The letter bears the salutation, " To a most skillful physician; Corypheus of Anatomists; Regius Professor Anatomy and Botany in the University of Paris; Dean of the same University and First Physician to the Queen, mother of Louis XIII, of France." Harvey also referred to the well-known Frenchman as the " celebrated Riolan," an adjective justified even today by the survival of such terms as Riolan's muscle and nosegay. Riolan is remembered also for his denial of the occurrence of the psoas minor muscle in the female, and according to Hyrtl, should also be remembered for the introduction into anatomical terminology of incorrect forms from the Greek, such as psoas magnus and parvus. Since these very terms still are in current use, Hyrtl's assertion that such linguistic nonsense never was preserved so faithfully or perpetuated so long by all the world was fully justified.

Since Harvey's second was not occasioned by a reply from Riolan

to his first letter, the former must be regarded merely as a continuation of the latter. Both letters were issued at Cambridge in 1649, the first following the receipt by Harvey from Riolan, of what the former termed ". . . a small anatomical and pathological work"—the *Encheiridium Anatomicum et Pathologicum*, a duodecimo volume published in Paris, in 1648. Harvey's letters hence were written twenty-one years after the publication of the *De Motu Cordis* and only two years before the appearance of the *De Generatione*. His surprising words, therefore, cannot be waived aside as based merely upon hopes, for at that time he either actually was, or soon was to be, engaged with the final revision, for publication, of his embryological treatise. The estimate of his studies on generation which he then made hence not only must represent his matured, but also his final, judgment. Moreover, it may be assumed that everything said in these letters to Riolan was considered carefully.

According to the *Opera Omnia* of 1766, Harvey wrote as follows: ". . . aperte si loquar, haec ita esse (ut vulgo recepta sunt) non opinor: et multa sunt, quae me in eandem sententiam ire suadent, in generatione partium observanda; quae hic recitare non expedit. Propediem forsam miraculosiora, majoremque lucem naturali philosophiae allatura: quae a me in publicum dabuntur." (p. 136) In order to forestall possible criticism of the translation, or the inference of bias on my part, my colleague, Professor Harriman, has kindly made the following literal translation of the above words. "If I should speak openly, I do not think that these things are thus (as they are commonly taken); and many things are to be observed in the generation of parts which lead me to the same opinion; these things it is not expedient to recite here. Shortly, perhaps, more miraculous things and which will bring greater light to natural philosophy; these will be given by me to the public."

Although Lewellyn who first rendered this expression into English in 1653, only four years after it was made, used a somewhat freer wording, the sense, to be sure, remains exactly the same, for his translation reads as follows: "If I may speak freely, I do not think that these things are so (as they are commonly believed) for there are many things which persuade me to that opinion, which I will take notice of in the generation of creatures, which are not fit here to be re-

hearsed; but it may be things more wonderful than these, and such as will give more light to natural Philosophie shall be publish'd by me" (p. 186).

Since the translation by Willis has been found reliable on the whole, and in order that readers may judge these words in their setting, I shall add the entire paragraph in which they occur as translated by Willis, almost a century later, in 1847, italicizing the words concerned.

"Still further; from the summary which precedes, and by way of satisfying those who are importunate in regard to the causes of the circulation, and incline to regard the power of the heart as competent to everything—as that it is not only the seat and source of the pulse which propels the blood, but also, as Aristotle thinks, of the power which attracts and produces it; moreover, that the spirits are engendered by the heart, and the influxive vital heat, in virtue of the innate heat of the heart, as the immediate instrument of the soul, or common bond and prime organ in the performance of every act of vitality; in a word, that the motion, perfection, heat, and every property besides of the blood and spirits are derived from the heart, as their fountain or original (a doctrine as old as Aristotle, who maintained all these qualities to inhere in the blood, as heat inheres in boiling water or pottage), and that the heart is the primary cause of pulsation and life; to those persons, *did I speak openly, I should say that I do not agree with the common opinion; there are numerous particulars to be noted in the production of the parts of the body which incline me this way, but which it does not seem expedient to enter upon here. Before long, perhaps, I shall have occasion to lay before the world things that are more wonderful than these, and that are calculated to throw still greater light upon natural philosophy.*" (p. 36).

Although these startling words were uttered when Harvey was considering the function of the heart, that is of the ventricles, and of the circulation, and while he was controverting some ideas of Aristotle and others, because of what he had observed regarding "the generation of parts," "the generation of creatures" or "the production of the parts of the body" as the different translators render it, the particular unpublished treatise which he then had in

mind remains a matter of inference. It hence may seem to some that Harvey must have had something else than his *De Generatione* in mind, but this is highly improbable, for the other manuscripts which are known to have remained unpublished at that time were that on muscles; on the function of the spleen; on respiration; and his "Medical Observations," or "Medical Observations and Pathology," or "Medical Anatomy," or "Anatomy in its Application to Medicine," as he variously called it. In his *De Generatione*, Harvey further mentioned "our Physiology and particular treatise on the Blood," (see page 498) and also "a section of Physiology," (page 518) but these treatises apparently never were written. It will be recalled that in the 68th chapter of his *De Generatione* he also mentioned the loss of a treatise on the generation of insects, during the political disturbances of 1642. These oft-quoted words read:

"And whilst I speak of these matters, let gentle minds forgive me, if, recalling the irreparable injuries I have suffered, I here give vent to a sigh. This is the cause of my sorrow:—whilst in attendance on his majesty the king during our late troubles and more than civil wars, not only with the permission but by command of the Parliament, certain rapacious hands stripped not only my house of all its furniture, but what is subject of far greater regret with me, my enemies abstracted from my museum the fruits of many years of toil. Whence it has come to pass that many observations, particularly on the generation of insects, have perished, with detriment, I venture to say, to the republic of letters." But, having been lost, this treatise manifestly could not have been the one concerned, and the context of the paragraph in which Harvey referred to something unpublished as well as the titles of the other unpublished treatises seem to exclude all except the *De Generatione*. They who feel that the evidence in support of this conclusion is inadequate face the dilemma of having to admit that some treatise of lesser merit must have impressed Harvey as more wonderful and more illuminative of natural philosophy than his "Libellus Aureus." Such a conclusion necessarily would have to rest on less convincing evidence, and would be more surprising still. It may also be recalled that in the Preface of the Works of William Harvey (1847), Willis declared that he included therein ". . . everything of our illustrious country-

man worthy of publication" and added in a footnote that ". . . a manuscript comprising Harvey's first course of lectures given at the College of Physicians on the Heart and Blood" was not in existence at the time. Willis also mentioned a manuscript on the "Muscles, Vessels and Nerves and on Locomotion of Animals" and of a book of Receipts or Prescriptions, partly contemporary, as containing notes, but not in Harvey's hand and concerning cases that occurred after Harvey's death.

After further consideration, it seems less surprising to me that Harvey should have underestimated the value of his demonstration of the circulation than overestimate the significance of his work on generation. He could not possibly have realized the far-reaching effects of the former. The knowledge of the structure and function of living organisms was entirely too inadequate at the time and his triumphant demonstration of the circulation of the blood hence probably seemed little more to him than the final establishment of a fact, otherwise of no supreme significance. Discovery of the genesis of an organism, on the other hand, was of surpassing importance and interest. But, be that as it may, the above words make it undeniable that Harvey regarded something which he was about to publish as considerably more important than what he was discussing with Riolan in 1649, and hence, by implication, also more important than the immortal *De Motu Cordis* of 1628.

It may also be emphasized that Ent scarcely could have remained unfamiliar with Harvey's letters to Riolan and hence was fully aware of Harvey's high opinion of his embryological studies, through them, if not also through personal contact with Harvey. This makes it easy to understand why he entreated the publication of the *De Generatione,* and why he thought of himself as returning with the "Golden Fleece," on that Christmas Day in 1650 when Harvey had entrusted to him the manuscript of the *De Generatione* with full authority to do with it as he liked. In view of these things it seems unlikely to me that Harvey ". . . would have delayed" the publication of his ". . . large number of observations," on generation, as Pye-Smith thought, and Willis probably was quite correct in saying that Harvey was ". . . rapt in the contemplation of the mysteries of Generation," even if it is doubtful that he actually could have been ". . . coolly engaged with his book" while ". . . under a hedge and within reach of shot."

However, that Harvey took the revision of his manuscript of the *De Generatione* seriously is known from his letter "To the Excellent John Nardi de Florence." This is dated "the 15th of July 1651," and in it Harvey said, "I should have sent letters to you sooner, but our public troubles in part, and in part the labor of putting to press my work 'On the Generation of Animals' have hindered me from writing." In a letter to Paul Marquard Slegel of Hamburg, "Written in London, this 26th March 1651," he further said, "I was myself preparing a sponge for his most recent arguments; but intent upon my work 'On the Generation of Animals' (which, but just come forth, I send to you), I have not had leisure to produce it." Although Harvey was well aware of the speculative nature of some of his views and frankly acknowledged that he had ". . . come to a stand" on some matters, he certainly would not have labored at the revision of the work or have sent Slegel a complimentary copy of it, if he had not thought well of it himself.

I admit that it is difficult to decide what in the *De Generatione* Harvey regarded as ". . . more wonderful" than anything he had revealed or considered in connection with the heart and circulation and which was ". . . calculated to throw still greater light upon natural philosophy." It may seem idle to speculate, but it can be said that although Harvey knew that he had not proven that all living things arise from ova, as we know them, he nevertheless felt that he had established that all higher organisms arise *de novo in utero*, from nothing corporeal, but through an influence exerted, directly or indirectly, by the male upon the female organism. He further thought that he had shown the earliest evidences of the primordium, or ovum, of the new being that can be seen in the form of a web in the uterus, about two months after coitus, in ungulates, and that in birds, such as the hen, the chick arises from a nonentity, the vital principle, in the colliquament of the cicatrix. He further concluded that the parts of the body, or organs, appear in succession, the blood before the heart, the latter before the liver, etc., and that the blood possesses an inherent power of pulsation.

Harvey's surprising evaluation of his *De Generatione* becomes more comprehensible in the light of the state of knowledge of physiology in his day. It is not unlikely that the chief basis for his lower evaluation of the *De Motu Cordis et Sanguinis* lies in the many

insurmountable difficulties which faced him when he attempted to visualize the connection of the arterial and venous systems and tried to explain the uses of the circulation. This is supported by the following words from Curtis (1915):

"So we find Harvey in his old age induced by lifelong study to question, if not deny, even the cooling effects of respiration, and to end with a practical confession of ignorance. Instead, therefore, of the circulation and its swiftness being explained by the urgent need of 'the substance of the air' experienced by certain tissues, that movement of the whole mass of the blood through the lungs, which was so novel a physiological fact, does not seem to have affected his view of the problems of respiration. Nor could he properly explain the respiratory change in the color of the blood, which seemed to support the ancient doctrine that the blood is of two different kinds. Since he could not invoke respiration to elucidate the circulation and its rapidity, and since he himself declared that such rapidity could not be needed for the simple feeding of the tissues, what was left to be invoked? It is no wonder that eight years [1] after the publication of his discovery Harvey denied that he had ever seriously undertaken to explain the use of the circulation; that at the end of thirteen years more he repeated this denial in his old age; [2] although he had not refrained from expressing such conjectures as must always be evoked in the mind of a great observer by a discovery of the first importance made by himself. Yet the phenomena of the very circulation used were so striking as to cry aloud for elucidation; for Harvey's own clinching statement that the heart drives into the aorta at least one thousand drachms of blood in half an hour,[3] this *reductio ad absurdum*, which cut the ground from under the feet of his opponents, left him helpless in his turn to account for the need of so huge a flooding of the arteries.

"Since it was not to be swiftly altered in the lungs that the whole mass of the blood hurried back from all parts of the chest, what then?" (page 40).

[1] Harvey: Letter to Hofmann, Syd. 596, l. 3-7; Op. Omn. 636, l. 1-4.

[2] Harvey: Exercise to Riolanus, II, Syd. 123, l. 15-18; Op. Omn. 122, l. 31-123, l. *l.*

[3] Harvey: On the Motion, etc. IX, Syd. 48, l. 21-50, 1: 36; Op. Omn. 50, l. 17-52, l. 23.

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CONTEMPORARY FOLK-TREATMENT OF STERNUTATION

LEO KANNER

Since the publication, in 1931, of a comprehensive historical and ethnologic review of the beliefs and customs connected with sneezing,[1] a unique opportunity provided me with original documents bringing the material up to date.

A 13-year-old girl, named Mary Margaret C., accompanied by her mother and a press agent, arrived at the Children's Department of the Johns Hopkins Hospital for treatment of her incessant bouts of sneezing. For approximately two months, press bulletins and newspaper headlines had kept an anxious public informed daily about the progress of her paroxysms. Less than two days in a private room, emphatic invitation to the gentlemen of the press to look for other hunting grounds, a plan for better scholastic and vocational adjustment, and reassurance about her unwarranted fear of tuberculosis quickly changed Mary Margaret from a sneezer to an ex-sneezer. In her unbounded gratitude, she did two things. She wrote a rather naive and bumpy poem commemorating her cure, and she turned over to this writer for temporary use a collection of cards and letters which she had received from all over the country.

An analysis of this correspondence furnishes good insight into popular views and practices in our present civilization regarding the etiology and treatment of sternutation.

Of the 118 letter writers, 51 were men and 67 were women. Most of them reported that they were or had once been similarly afflicted or that some relative or neighbor had sneezed incessantly until cured by the remedy now placed at Mary Margaret's disposal. A few letters came from anonymous senders, many were accompanied by the request that the writer be informed of the results of his or her suggested treatment. Though most notes came from " just plain folk," there were among the advisers two physicians, one recommending the inhalation of chloroform " under the supervision of a doctor," the

[1] Kanner, Leo, " Superstitions Connected With Sneezing." *Medical Life* (1931), 38, 549-575. Extensive bibliography.

other urging a few hypodermic doses of cicutine hydrobromide: "It is the only thing that will help you. Don't delay. Do it at once." A homoeopathic M. D. from the Nation's Capital offered his help, asserting that he had been instrumental in the cure of "the famous yawning lady" from Vancouver, B. C. Six chiropractors went out of their way to write to Mary Margaret's parents. One invited the patient to come to see her at her office in Washington, D. C., saying: "I am a chiropractor, osteopath, basic technician. I have several methods I can use that are very effective." Another, from Alexandria, Va., enclosing circulars, admonished the parents "not to take any further chances with hospitals, etc." but to bring the child to him and he will stop her from sneezing; if not, "it will not cost you one penny." The head of a chiropractic Health Institute in California wrote: "Have been reading reports of your daughter's condition, I have had similar cases, the cause is obvious. Am widely known here and enjoy a large practice. I am sure I can do something for your daughter." A lady D. C. from Jackson, O., advised: "I am a Chiropractor myself and I believe if you would try a Chiropractor he might be able to do her some good. It would be worth trying anyway." A chiropractor's widow raised her voice of experience: "Of course Doc don't like chiropractors but incest (*sic*!) on having one see the girl." Other writers identified themselves as a student and teacher of metaphysics, a country school teacher, a maker of patent medicine from Tampa, Fla., a Beauty Shoppe owner, a minister, an analytical and consulting chemist, and a man "with a good college education in chemistry." Not less than 16 letters were addressed to the Johns Hopkins Hospital, all others either to Mary Margaret herself or her parents. One dear lady started off cheerfully with the words: "If you are still living," and another: "Perhaps this letter will never find you."

Almost all of the writers were prompted by a spirit of helpfulness. One only had no advice whatever to give to Mary Margaret. As one sneezing girl to another, she merely furnished a vivid and heart-rending account of her own sufferings and told how she had even offered her head "for Dr's to studdy when I die." The doctors, with utter lack of scientific curiosity, failed to seize upon this golden opportunity and declined the maiden's sneezing head.

Etiology

Often, in addition to the remedies suggested for Mary Margaret, the missives contained expressions with regard to the possible etiology of her excessive sternutation. The authors were quite certain that they knew the cause. Not so the host of newspaper reporters who kept calling on hospitals and physicians for enlightenment which they might transmit to a puzzled public. One reporter, told by a doctor friend that the condition had possibly arisen on an allergic basis, turned his copy over to his editor and the next issue of that venerable daily newspaper broke out with the following remarkable headlines:

THIRTEEN-DAY SNEEZING SPELL OF GIRL
AN "ALLEGORICAL MANIFESTATION," DOCTOR SAYS

A local cause in the shape of an irritating "wild hair" was suspected by three advisers. An Ann Arbor, Mich., woman was "just a wondering if a small hair in the lining of the nose could possible be the constant sneezing." A lady sympathizer from Eldorado, Ill., also felt that "maby some wild hairs that have growed in her nose" were responsible. A revealing case contribution came from a Stillwater, Minn., epistolographer whose grandfather had been similarly afflicted: "It was found that a wild hair (as they call them) was growing in the nostril and it kept tickling the inside of his nose which caused him to sneeze. After they took the wild hair out of his nose his sneezing ceased."

Five writers suggested a neurological origin. Said a Penns Grove, N. J., sage in a letter to the Johns Hopkins Hospital: "Dear Sirs—Concerning Sneeotis, or Snee-no-tice. I would advise you to write (WIRE) to the Mayo Hospital. I will not use 'technical' or mestical (terms). I advise you to make extensive X-ray examination of the nose between the eyes, mostly near the ridge of the nose. There is a nerve there that will cause any-one to sneeze every-time it is touched, even if it was 100 times an hour. You may find something has gone wrong with it, Injury, faulse groth, or infection." A D. C. gentleman, possessed of a remarkable memory, communicated his experience to the Hopkins doctors: "I know not what will cure your patient from excessive sneezing—but I do know that here in Washington about 35 yrs. since a young lady neighbor was similarly

afflicted. Doctors could do nothing. Finally the nose was opened—two nerves were found crossed. The condition was corrected with complete success." A Kankakee, Ill., chiropractor's widow was sure that "it is only a pinched nerve and if the pressure is removed the girl will be well." The guilty nerves are not necessarily located in or near the nose. Consider the amazing case of an Englewood, Colo., chiropractor who "found a nerve congested on the bottom of the foot which acted on these sneeze glands in the throat, they thought it would run into consumption. Congestion was relieved and patient stopped sneezing; Mayo Bros. could not help." An anonymous D. C. lady, impelled by the spirit of service, imparted her knowledge to the Hopkins people thus: "About sneezing. I sneeze owing to an itching rectum, when I aply a soothing, or would be soothing, pad, of coton, saturated in an antiseptic, composed chiefly of alcohol, or even cold water. Surgeons probably know of the relationship of such nerve centers. There may be some such cause and possible the child, Mary Margaret C., may have a slight suspition of it, but to sensitive to tell, or it may be her changing age may affect such nerves. It seems to me the nose has been so thoroughly trted, the trouble must be located elsewhere, in some conecting ceter, an X ray might aid. Pardon me for taking your time. I am a grown woman and have not the courage to mention my own experence to any one. I sneeze until I relieve the *cause*. I hope she may soon be relived."

A Coral Gables, Fla., "Creator of Styles" averred that "this lettle girl's condition is caused by toxemia and acid in the Systum" and consequently should be treated by "keeping away from her all meats and citrus fruit juices and giving her high Clernic eregations."

Dental causes were accused by three volunteer helpers. A Takoma Park, Md., matron wrote: "When I was a girl in my teens living in Baltimore I was stricken with sneezing spells. Nothing could be done to stop them. I Became very much run down, at last at my own suggestion and determination I insisted and begged and prayed that I might have all my upper teeth taken out. As a young girl that was thought to be dreadful thing but at last was able to find a dentist in Philadelphia who extracted all of my upper teeth. I never had any more sneezing spells, and I am now a mother of middle life and have found my false teeth a great comfort and no trouble or burden. I

could tell more about if I was talking about it to you, but please insist on this. Feel her gums and see if they are even a little inflamed. Hopefully yours, A Mother." A Pittsburgh, Pa., girl who had "the same thing" for three years was likewise cured by having her teeth pulled. A Chicago ex-sneezer, asserting that "every tooth has a sinus and if these are affected all other sinus is more or less troubled," had this to say: "Please go to a good head surgeon. One of the sinus of your teeth may be irritated and you are pulling air up through your tooth sinus which agrivates the nerves of your nose. If you had a tooth pulled lately there may be a direct opening into your tooth sinus which in my case caused me to sneeze violently for months. Mine was a broken off wisdom tooth. An X-ray might do the trick to find which tooth broke into the sinus. A well wisher."

A British lady, who had been a terrible sufferer from sneezing so dreadful that no doctor could cure it, was certain that "it is only the liver." She had learned that to her own advantage from a very clever nurse. "So I do recommend you to attend to your Liver and forgive a stranger for writing, but I have suffered agonies."

"Minnie" from Toronto had a theory of her own: "I see your condition of sneezing record in the news papers. I say unto thee dear peace. be still and rest. Your condition is but a record of prewritten history recorded in the Bible. But from to-day you should be at rest. You lived in the time of George Washington. God bless you and keep you. love from Minnie. Read your Bible." And from Luverne, Minn., came this message: "Friend in the Lord, as are people of the Mother Earth Spirit on the New Earth are called Saints of the Earth Revelation New Test of Bible and they overcome by the word of their testimony from the Word of God thou their Angel who beholds their their Father's Face in heaven to drive out any enemy when God gives husbandry over to another in Testing time. By except the Christ came to redeem man from sin and he came the Sec. time according to Rev. so by excepting the same than according the Book of Mathew you will see the Kingdom of God come down with great power and ye shall see Angels decending and ascending upon the Son of Man. See Rev to find out who the Angels are and the Saints of Earth by uniting with the Saints of Heaven. It brings one faith, One Lord, One Baptism. Your test is if you accept the whole Armor

of God as St. Paul said then brings in the Errors of the Wicket so called by returning in faith to inhabit the earth in the regeneration of man. These are called Jesus Angels that is the Saints of the Earth as the Saints of Heaven are not subject unto the body of Christ according to Scripture. Out of the nostrils of the wicket shall come fire and brimstone to devour their enemies bringing in the Law of God manefest in the Flesh to become Jesus redeemed in Spirit. Another edge of God's sword might mean I will bring a curse upon Dan's horses which come snorting. Yours in the Lord, Charles G. E. . . . ."

A Detroit, Mich., student and teacher of metaphysics desired some pertinent information before she would express herself definitely concerning the etiology. She wanted to know Mary Margaret's birthday, so that she might " set up an astrological map in an effort to ascertain the cause of her ailment."

A Cumminsville, O., man, in a letter to the Superintendent of the Johns Hopkins Hospital, generously offered an etiological theory, a remedy, and a free sample. Wrote he: " The reason physicians are unable to stop the sneezing of Mary Margaret C. is that they have no means of controlling inaccessible inflammation. When influenza was epidemic here in 1919 no treatment known to the profession controlled or sensibly mitigated the sneezing. A druggist sold an antiphlogistic for external application that stopped the sneezing within half an hour—rendering the disease a trifle in many families that used it. The preparation is applied to the forehead, bridge of nose, back of ears, temples and even top of head in some cases. Instantly attacks the inflammatory condition, however deepseated. As inflammation is the local feature of practically all the common ills and is not affected by drugs taken internally unless they occasionally come into contact with the part affected, an external auxiliary is appropriate. I enclose enough for one trial, with no expectation that you will touch it, as it is of secret composition. We used it successfully for 2 cases of pneumonia in our family."

Treatment

The principal therapeutic recommendations group themselves into the following main categories:

1. Suggestions, general or specific, of healers whom the patient should consult.
2. Metaphysical cures: Prayer, mental equilibrium, magnetic treatment.
3. Cure through fright.
4. External remedies: Pressing and rubbing.
5. Local applications to the nasal mucosa.
6. Spraying, inhaling, snuffling, smelling and gargling.
7. Exhaling.
8. Remedies per os.
9. Complicated recipes.
10. Hygienic and dietetic measures.

One gentleman from Reading, Pa., was not satisfied with just one type of remedy. He offered a choice of four, which he communicated "sincerely" to the Johns Hopkins Hospital staff. Here is his letter: "Gentlemen: Regarding the Girl mary margaret C. who suffering on Sneezing I suggest try following home treatments under your care. If any treatment as stated will help kindly let me know this in confidence. Thank you.

"1. Inhailing of pure Carbonic acid.
"2. Leeching.
"3. An Anesthetic in the Rectum.
"4. A small dose of her own Urine."

1. *Chiropractors, Osteopaths, Faith Healers, et al.*

A number of sympathetic souls, having no remedy of their own to offer, hastened to tell Mary Margaret whither she should turn for help. "No matter how many doctors you have tried do try a good Divine Healer," a housewife of Kelly Station, Pa., urged our patient. A Knoxville, Tenn., woman suggested "a neuropath doctor if possible." One D. C. matron sent a list of Christian Science practitioners; another, enclosing three specific addresses, recommended that our young sneezer go to "a good osteopath or chiropractor, as I have seen them do wonders in similar conditions. At least they could not possibly do any harm, and I have little confidence in the medicos with their serums etc. who are experimenting on your

daughter." A third lady from the Nation's Capital praised a certain "doctor who combines the methods of osteopathy, chiropractic, naturopathy, medicine and good old common sense." A country school teacher from Victor, La., advised the services of an osteopath, reporting that one had cured her cousin of sneezing. A Detroit, Mich., teacher suggested treatment "by a chiro or osteopathist, as I read a couple years ago about a Chicago Girl who sneezed for a while and she was cured by one or other above men; I am neither of them." Another woman from the cradle of Ford automobiles recommended trying a chiropractor, "one who gives basic treatments." Vertebrae adjusters were also lauded by a Washington, Ind., theologian and a Louisville, Ky., matron who was stopped by one from sneezing within five minutes: "For conditions like your daughter's he rubs." A D. C. lady announced tersely that she herself had been cured by a "specialist." From far away England came the plea that the patient try to obtain help by writing to a certain herbalist in Sheffield; when doctors of renown had given up the writer's daughter sixteen years before, that man gave her some medicine that brought instant relief.

2. *Metaphysical Cures*

Prayer. Faith and prayer were recommended by a Richmond, Va., woman. One from Tampa, Fla., announced that she kept praying for Mary Margaret night and day. One from the Nation's Capital sent a copy of "The Sword of the Spirit, God's Witnesses to the Pure First Century Gospel." From the same center of government and learning came also this message written by a man too modest to sign his name: "I am mailing under separate cover a jar of salve. Insert a piece as large as a bean up each nostril. Press nostrils together, afterwards inhale naturally. This will heal the membranes. Blow the nose naturally when mucous drops. Do this 3 or 4 times a day for 3 days. Every morning at 10 A. M. I shall pray with you for 15 minutes. Ask mother to sit silently and pray also. Concentrate during that period on this statement: 'Infinite love is every where present, and has power to heal as nothing else can.' Then give God thanks by saying: 'Peace, Father, I thank thee, It is wonderful (repeat several times). When you are healed, notify the

'Star' paper of Washington. Then I shall come to you. You shall begin to improve with this letter."

Mental equilibrium. Word came from an Alabama sage: "The best way to keep from sneezing is to keep the eyes open and press the upper lip under the nose. And to pass the time and make you alert mentally, repeat or write the alphabet backward, also the multiplication table backwards. Also transpose the alphabet in 1-3 2-4 order like this A C B D — E G F H — I K J L — M O N P — Q S R T — U W V X — Y Z Do this forward and backwards. And in this manner, Az By Cx Dr Ev Tu Gt Hs Ir Jo Kp Lo M N. The results will seem nothing less than miraculous, you will amaze both yourself and your friends. And if you become further interested in Mental equilibrium and nerve stability, the course will be worth many times its cost to you."

Magnetic treatment. From Hornell, N. Y.: "My dear Child, if you are still living let your mother lay this (?) on your flesh under your clothes and pin it so it will stay open better. This is a magnetic treatment. From Mrs. T. C.

"P. S. To the Nurses: Don't handle this any more than you can help. Slide it to pit of stomach and to the hollow of neck.

"P. S. Remedy: Warm a little milk and wet her tongue and lips and eyes and ask her to open her eyes and wake up if she is living. Keep this to yourself, keep her quiet and no visitors, hope you won't think me rude in writing to you."

3. *Cure Through Fright*

A succinct note, addressed to the Johns Hopkins Hospital, came from Mr. R. W. of Los Angeles, Calif.: "Gentlemen: Try to frighten Mary Margaret C. who is sneezing, this may relieve her."

4. *External Remedies*

Pressing the upper lip. Not less than twenty-five writers obtained and advised relief from sneezing by pressing the upper lip just below the nose. A Paris, Va., woman has done this often "to prevent sneezing in church and it always worked." This recommendation came from matrons residing in Baltimore, Md., Charles Town and

Martinsburg, W. Va., Brainerd, Minn., Beaumont, Tex., Gardenville, Va. ("I always do that if I am in church and it is a success"), Washington, D. C., and from men dwelling in Delaware, O., South Enola, Pa., Vineland, N. J., Silver Spring, Md., Goodwater, Ala., Portland, Me., Los Angeles, Cal., and Washington, D. C. ("It feels funny till you get used to it, but it'll stop ordinary sneezing well"). A lady chiropractor practicing in Frederick, Md., added: "That is what soldiers do to repress a sneeze when they must keep silence." The busy owner of a Kinston, N. C., Beauty Shoppe, who had quite a few people sneeze while she treated them for acne, took enough time off from her work to send Mary Margaret generously "a piece of toilet paper folded to be put under the upper lip just as far as you can." From a Guilford, Conn., matron came the prescription: "Put your finger on upper lip and press and hold your breath." An experienced Indianapolitan used a pencil instead of his finger: "It sound silly but it has always releived me, by holding a lead pencil tight on my upper lip against the nose until it stops. You may have to hold 10-15 or 25 minutes." An Ironton, O., skeptic suggested placing the forefinger nail well trimmed close beneath the nose and pressing real hard but, uncertain of the result, added: "If this fails try a chiropractor doctor." A Hagerstown, Md., woman, equally believing in the need of reinforcement, followed her advice to "lay your finger on her upper lip as near the nostrils as possible and press hard" by this autobiographic bit: "I tried all kinds of physicians and here is the cure I did. I read the 14th chapter of St. John in the Bible. It is a great thing for God will heal when the specialists have failed."

Pressing other parts of the face. Therapeutic benefits may be derived from pressure on other parts of the face as well. A Birmingham, Ala., woman thought she was a "Marathon sneezer" until she heard of this new case. When the doctors had failed her, she "went to praying and ask God to deliver me from this damnible sneezing. After praying to God to heal me I received an impression to massage the forehead and around the eyes and rub down the nostrals and I received instant relief and I am thankful." A Rock Falls, Ill., matron wrote: "I want to tell you something that has always stopped me sneezing when in Public Places etc. and pass it on

to you it might help you. Press the thumb and finger tightly just below where the nose piece of spectacles fit on the nose in the hollow just below the bone and squeeze hard a few minutes this where its coming on. Always stops it for me."

Rubbing the wrist. A lady well-wisher from the City of Brotherly Love, having heard the tragic radio news about Mary Margaret's predicament, hastened to submit her "experience" to the "John Hopkin Hospital": "Rub her Wrist where the Pulse and blue veins are very quick up & down also towards elbow & when she feels spell coming do it before starts. In hurry to mail."

Rubbing with mustard or mustard plaster. "Yours Respecfuly J. H." from Washington, Pa., thus addressed the Hopkins faculty: "Dear sirs I heard over the Radio about the girl With hick up and thought of a simple remedy that is to take a Littel mustard and mix it With a little Pure hogs Lard and rub it on the Rist and ankels and on the Back of the neck and I believe it Will Check the hick up at once you know it is harmless. it is good in case of Convultion." A Poolesville, Md., man, the brother of "a train nurse who prescribed it," promised results from "mustard plaster on her stomach and ice on the back of her neck, then change after a few minutes, put mustard plaster on back of neck and ice on stomach."

5. *Local Applications To or Near the Nasal Mucosa*

Application of ice. A Virginian sent this letter to Mary Margaret's family: "You may think this letter a funny one from someone you never knew but I saw your picture in the paper and it interested me quite a bit as I had quite a spell of sneezing myself the way it took me was for about 10 to 15 minutes steady and about half an hour between times but my case only lasted four days and nights and there didn't seem any relief at all but there was an old man that told my people to take a piece of ice and hold it between the eyes on the bridge of the nose until it gets numb and believe me it didn't do the trick at first but we kept trying and it gave me relief and now don't think I am too forward in sending you this letter but I couldn't help but feel sorry for your little girl as my baby is just your age and I also realize what you are going through."

Cocaine solution. From a Des Moines, Ia., man came this message to the Johns Hopkins Hospital: "I read in the Des Moines Tribune of Mary Margaret C. sneezin 54 days. I have had sneezing attacks. And by useing a solution of 2% Cocaine. It stoped right away. If this has not been tried I would sugest 2% or stronger. Would you please let me know whether this any help or not."

Unguentine. A Farmington, N. C., ex-sneezer who wanted "to healp in solveing your problium" suggested the local application of "Unguentine" which had relieved him.

6. *Spraying, Inhaling, Snuffling, Smelling and Gargling*

Alcohol. A Tampa, Fla., chemist who had had the same ailment once found relief by simply inhaling pure grain alcohol. "Pour a quantity on a handkerchief and put it to your nose and hale it deeply and continue to do so until the alcohol fumes seem to smart or burn your lungs."

Borax and sugar. A recipe from "Grandmother Eve" of Fort Collins, Colo.: "One thimble household borax. Four thimbles powdered sugar, mix and run through wire tea strainer 3x. Snuff powder dry. Keep snuffing up your nose untill you can taste the sugar. Snuff real often."

Camphor. From Paterson, N. J.: "Powdered camphor should be sniffed." From Washington, D. C.: "Saturate a piece of gauze with spirits of camphor and have patient inhale it." Suggestion "of a Dr. who does not believe in medicine very much, but offers simple remedies": "In incessant sneezing with profuse running from eyes and nose, powdered camphor should be sniffed." This suggestion did not come directly from the doctor but in the form of a quotation forwarded to Mary Margaret by a Clifton Forge, Virginia, benefactor. From Kansas City, Mo.: "Inhale fumes of Camphor Gum, burn a piece in a cup, place a bath towel over head, blow the light out and inhale the fumes. Allow to burn only a second or two. You may have to try several times."

Catarrh remedy. A Kenmore, N. Y., benefactor sent a generous sample of a "catarrh remedy" to the "Doctors Attending Miss M. C." with the following comment: "It will make her sneeze and

then heal up the membranes. Perfectly harmless. I have nothing to sell and am doing this as a Humane Act."

Fumes of smoking wool. The Johns Hopkins Hospital received this letter from a male resident of its own hometown, Baltimore, a carpenter by trade: "I just read the pathetic state of a 13 year old child, having sneezed for 55 days. I too have had sneezing spells—not for such long periods, however. For years now I have found this to be a relief and cure. You might try it upon the child. Certainly it is harmless. Strike a match to a piece of woolen material and while this small piece of goods is burning, pour some granulated sugar on it and hold this piece of material to her nose. The fumes of this smoking piece of material as she inhales it, I'm sure will help to relieve this child."

Garlic. A man who had transferred his domicile from Poland to Buffalo, N. Y., delivered himself of this remarkable epistle: "please if this sneezes is egzist till now then give to the girl luke warm milk first boil then kool of so the same be luke warm by this time give to smel something so shi she not smel before strong flavor razes lik or garlik watch so shi get noff air in organism shi must keep in, best get in and keep till must trow out this min breath in and keep air in give dring milk till shi can dring so the sneeze space be full but be best let shi see what shi will dring. the same way can be use to kill of hiccough this way whose in use in old Country."

Horse-radish. From Holly Grove, Ark., came this piece of advice: "Get some horse radish and grind up to fill a one half pint jar and cover it with vinegar and inhale same."

Listerine. A contribution from Taylorville, Ill.: "Listerine to be snuffled up your nose, don't be afraid of it. Gargle with it many times during day and when awake at night."

Olive oil. Two sympathizers from Philadelphia and an Atlanta, Ga., man recommended olive oil as a cure of sneezing. One of the letters said: "Snuffle olive oil into your head through the nostrils." The second, sent by airmail, special delivery, declared: "Miss C. may possibly get relief from sneezing by a couple of drops of olive oil as soon as she feels the sneezing-sensation coming on (rubbing

the outside as well as the inside of her nose)." Mrs. S. P. was much more specific. She gave the following detailed instructions in an epistle addressed to the Johns Hopkins Hospital:

"To Whom This May Concern:

"Having just read of Mary Margaret C., case of sneezing it remined me of the time I had a sneezing spell.

"Although it wasn't has bad has Mary's, and I thought it due to a Cold, it was very un-comfortable.

"Never-the-less I tried some-thing which stopped my sneezing.

"This may sound silly to you and may not work in Mary's case, but there is *no* harm in trying, so I'll tell you what to do:—

"Get '*pure olive oil*';

"Pour some into small pan, and let come to a *boil.*

"Has soon has it boils, shut off gas.

"Wait until it is *luke* warm, then put into nose dropper.

"Three (3) drops in each of 'Mary's nostrils.' Make sure her head is held back enough so's drops goes in.

"After through with that allow her head to *remain* back for a few seconds.

"Then relax, *bath* the *back* and *front*, of her neck, with same oil *heated* again hot enough for her to stand it.

"Bath for about *five* (5) minutes, back and forth.

"Repeat this treatment every half (½) hr. 'But each treatment must be made fresh.'

"So put enough in pan, that you think you will need for each treatment.

"None will be wasted if you do that.

"I do hope you. will understand what I've been trying to tell you, and also hope you will try it, has it can't hurt her, and it *might* do her good.

"I truly hope it does.

"If you'd care to write me, after you've tried this, I'd appreciate it ever so much.

"I shall be waiting anxiously for a reply, within the week if it isn't to much bother, and please let me know if it works. Thank you.

"P. S. I forgot to say that the treatment, should prove resultful with-in-a day, if it works her like it did me. Write and let me know regardless. Thank you again."

Peppermint. This from Rock Falls, Ill.: "Try essence of peppermint to stop sneezing. It may help, I know for an ordinary cold it acts like magic, will stop sneezing and coughing. About ¼ teaspoon full essence peppermint in one glass of water, just gargle, or sip and swallow slowly. Every few minutes or constantly until relieved. A little baking soda added helps too. I hope you are better tho. This is a good remedy for sore throat too."

Pepsodent and Vicks cough drops. From Washington, D. C.: "About one month ago I have a sneezing spell like yours, a tickling in the throat. I used Papsidont to gargle my throat and Vicks cough drops, two very simple remedies. But they cured me, I kept a cough drop in my mouth all day; as one would dissolve I would take another. Try these please."

Red oak bark. From a lady inhabitant of Baltimore's aristocratic residential section: "Please ask the Drs. to try Red Oak Bark. Get a package at the Drugstore, brew as you would tea. Put drops in the nose often, gargle the throat. Hoping you find relief."

Salt solution. A woman ex-sneezer from Washington, D. C., sent this remedy: "¼ of teaspoonful of salt put in cup of hot water just hot enough not to burn inside of nose too much. Snuff up nose and let it run back through mouth 3 or 4 X. Try this every time you have a sneezing spell."

Smelling salts. An "almost blind man" from Ivyland, Pa., wrote "to the Doctors": "A suggestion for the patient suffering from sneezing. I have been subject to spasm of sneezing, of lengthy duration for several years and found that a whiff of smelling salts unvaryingly stopped them as soon as used. Also the paraxysms are fewer and less now. Try it and may it have the same effect."

Snuff. A brief message from Modesto, Calif.: "Put some snuff into the nose."

Sprays. A Washington, D. C., woman sent an unidentified spray which had cured her from sneezing. A New York City man wrote:

the outside as well as the inside of her nose)." Mrs. S. P. was much more specific. She gave the following detailed instructions in an epistle addressed to the Johns Hopkins Hospital:

"To Whom This May Concern:

"Having just read of Mary Margaret C., case of sneezing it remined me of the time I had a sneezing spell.

"Although it wasn't has bad has Mary's, and I thought it due to a Cold, it was very un-comfortable.

"Never-the-less I tried some-thing which stopped my sneezing.

"This may sound silly to you and may not work in Mary's case, but there is *no* harm in trying, so I'll tell you what to do:—

"Get '*pure olive oil*';

"Pour some into small pan, and let come to a *boil*.

"Has soon has it boils, shut off gas.

"Wait until it is *luke* warm, then put into nose dropper.

"Three (3) drops in each of 'Mary's nostrils.' Make sure her head is held back enough so's drops goes in.

"After through with that allow her head to *remain* back for a few seconds.

"Then relax, *bath* the *back* and *front*, of her neck, with same oil *heated* again hot enough for her to stand it.

"Bath for about *five* (5) minutes, back and forth.

"Repeat this treatment every half (½) hr. 'But each treatment must be made fresh.'

"So put enough in pan, that you think you will need for each treatment.

"None will be wasted if you do that.

"I do hope you. will understand what I've been trying to tell you, and also hope you will try it, has it can't hurt her, and it *might* do her good.

"I truly hope it does.

"If you'd care to write me, after you've tried this, I'd appreciate it ever so much.

"I shall be waiting anxiously for a reply, within the week if it isn't to much bother, and please let me know if it works. Thank you.

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Sprays. A Washington, D. C., woman sent an unidentified spray which had cured her from sneezing. A New York City man wrote:

" To stop sneezing take a drop of viscid juice of turpentine from pine or fir trees such as they use in mixing paint use an atomizer to spray nose this will burn but at the same time it will kill the germ fungicide, not destroy. if it should burn so much cold water or ice will ease. Sincerely seriously scientific. R. T."

Terpezone. A Washington, D. C., woman suggested treatments with a Terpezone machine " that makes an ozone vapor to be breathed."

Witch hazel. From Biloxi, Miss.: " Pour small quantity of witch hazel in hollow of your hand, then sniff this up your nostrils until it reaches your throat. Repeat this several times. Sure it full strength. It never fails to relieve me in a few minutes."

Younce's Stop-it. Mr. J. L. Younce, Tampa, Fla., maker of Younce's Stop-it, having read in the papers that Mary Margaret had been treated without success by thirteen doctors, " one Specilist, one Charo," advised that she inhale Younce's Stop-it " up your nose deluted with ½ water." He continued thus:

" Also girgule in your throat full strength then swallow it. Repeat operation in 15 minutes—and you will fund your trouble is gone gone. I will gladly mail you a sample on request costs you nothing *only a Statement what cured you.* Please favor me with reply. You will notice a table sponful is a dose, yet it is harmless—a dozen Dos taking at one time will do no harm. In your case you should first girgle with a table spoonful in 15 minutes use ten spoonful that should be enough and if your trouble has not stopped in 40 minutes inhale twice more each time using ½ strength—and ust it freely until it reaches the troat—*and all I want is a Statement what cured you.* P. S. Younce's Stop-it removes eritation from the flesh, if it fails there is nothing elce known."

7. *Exhaling*

From a Washington, D. C., woman: " Take a small paper, such as is used in a grocery store, hold the open end closely around the mouth, so no outside air can get in, and breathe into it deeply a few times."

8. *Remedies per os*

Fruits and fruit juices. Two alternatives came from a Paris, Tex., matron in the form of suggested "reamendies": "1. One cup of pineapple juice, ½ teaspoon soda, dose of quennie. Mix all together in pineapple juice. If the first one fail take 2, 30 minutes apart. 2. Juice of one lemon, 1 cup of water and a tiny bit of alum. Drink while warm. Pinch of salt." Damson preserves were lauded by a Washington, D. C., woman. A Takoma Park, Md., housewife advised: "Get some pure Virgin Oil from Drug Store. Take juice of two lemons and add same amount of Virgin Oil to the lemon Juice add two teaspoonfull Boric Acid mix well and give Mary 1 teaspoonfull every ½ hour and I feel you will see a very big change in a very short time."

Bread. A Norfolk, Va., woman, offering her services gratis to the Johns Hopkins doctors, supplied the following remedy: "I read about your patient sneezing continuously and want to suggest eating a piece of bread, if that hasn't already been tried, I am subject to sneezing spells and I find relief for the time in eating a piece of dry bread, it has not failed to relieve me."

Corn fodder. A Sylvania, Ga., inhabitant sent this "very simple, old time remedy, which was give to us by old slave mammy and has cured my Brother and myself": "Take as much corn fodder as can meet finger and turn around hole blades, put in ½ gal of water and boil until it is good and strong, take a small tea cup with little seetrun, about every hour and take as hot as can stand it."

Garlic. A male denizen of Wilkes Barre, Pa., wrote: "I have been sneezing for quite a long time, and then I tried a remedy of eating a piece of garlic before going to bed. And now I don't sneeze no more and I hope this will help your daughter."

Carter's liver pills. A Maine maiden sneezer "immediately took Carter little liver pills and got quite well." She added: "This is not to advertize Carters, but they did me good."

9. *Complicated Recipes*

A Washington, D. C., woman sent this recipe:

" Bromide of Soda	120 grains
Tr. Hyoscyamus	120 drops
Oil of Cinnamon	10 drops
Elixir Lactopeptine	120 drops
Mixed.	

To be taken in 4 doses at one hour intervals."

A McAllester, Okla., matron, having suffered from hay fever for twenty years, was cured by the following remedy which she obtained from " a commercial salesman from Virginia ":

" Iodide of potash	4 drs.
Tincture of Belladonna	4 drs., and enough

Comp. Syrup of Sarsaparilla to finish filling a 6 oz. bottle. This concoction absolutely does the job and discontinues the sneezing. It's mostly nervous illness. Potash calms the nerves. Belladonna is a poison and combats the poison *in* and dries up the discharge."

10. *Hygienic and Dietetic Measures*

A Washington, D. C., man wrote a long epistle to Mary Margaret. He said of himself: " I am not a doctor though I have a good college education in chemistry, and I believe that the suggested remedy, namely *water and sleep*, can be satisfactorily explained from a chemical viewpoint." He recommended: " 1. Drink a lot of luke warm water to get temporary relief. 2. Get sleep. 3. Avoid perspiring. Avoid drafts or chills."

A British lady resident of King's Bench Walk in London wondered " if the same treatment which cured my sister from hay fever would cure you; it was to abstain from all fish, especially shell fish."

Conclusion

It is obvious that contemporary medical folklore, despite its seeming simplicity, is a highly complex mixture of a number of different ingredients. As this one example shows, the mixture consists of 1. retention of beliefs and practices dating back from the earliest days of civilization and prevailing throughout and beyond the middle ages; 2. " borrowed," though not fully comprehended, gleanings from

current scientific medicine; 3. survival of methods which were part of the repertoire of practicing physicians two or three generations ago; 4. references to present-day healing cults.

One feature forces itself particularly upon the observer. We might speak of it as the evidence of individualization in medical folklore. Etiologic notions and remedies used to be a matter of tradition, of wisdom handed down and taken over in the same form, rigidly adhered to by everybody. No "unhallowed hands" dared disturb the exact formulae. Many of the writers to Mary Margaret indulged in personal modifications, added a "personal touch" to the suggested formula. Firm conviction often gave way to some skepticism, at least to the extent that the writer urged that the remedy should be "tried," thus implying the possibility that the remedy might not work.

Thus, in contemporary folklore, we find a greater variety of "source material" than in older folklore, greater individual reliance on personal experience, individual modifications, and a sprinkling of skepticism.

TEXTS AND DOCUMENTS

THE *SPHERE OF LIFE AND DEATH* IN EARLY MEDIAEVAL MANUSCRIPTS

HENRY E. SIGERIST

1.

The *Sphere of Life and Death* was a magical device used for the purpose of finding out whether a patient would live or die. It could also be used for other purposes of divination, to ascertain whether a lost object would be recovered or not, whether an impending event would turn out happily or unhappily, and similar questions.

The Sphere was based on the magical virtue of numbers and astrology. It was easy to handle. All one had to do was to add the numerical value of the letters of the patient's given name. To this sum then was added the day of the moon on which the patient fell sick. The total was divided by thirty, and if the remainder was found among the numbers figuring on the upper half of the Sphere it meant that the patient would live. If, however, the number occurred on the lower half of the Sphere, the patient would die.

The Sphere was used by physicians because it is found in purely medical manuscripts. Undoubtedly it was also used by laymen because it frequently occurs in non-medical books such as missals. Charles Singer has very justly pointed out that such books were used by priests in visiting the sick and that they consulted the Sphere when they wanted to know whether the time had come to administer extreme unction.[1]

The oldest text of the Sphere preserved is that of the 4th century Greek Papyrus V of Leyden published by C. Leemans [2] and again by Albrecht Dieterich.[3] There it is attributed to Democritus, *Δημοκρίτου σφαῖρα, προγνωστικὸν ζωῆς καὶ θανάτου.*

[1] Charles Singer, *From Magic to Science*, New York, 1928, p. 146.

[2] Papyri Graeci musaei antiquarii publici Lugduni Batavi . . . edidit . . . C. Leemans, vol. II, Leyden, 1885.

[3] Papyrus magica Musei Lugdunensis Batavi . . . denuo edidit . . . A. Dieterich, *Fleckeisen's Jahrbücher für classische Philologie*, Suppl. 16, 1888, pp. 747-829.

This Greek Sphere of Democritus, although different, is related in character to the Sphere of Petosiris which seems to be of Graeco-Egyptian origin. It is included in an epistle of Petosiris to Nechepso and was published by Ernst Riess.[4]

The Sphere of Democritus was translated into Latin probably around the 6th century A. D. when such translations were made in large numbers. The translation, however, presented a serious difficulty. While all letters of the Greek alphabet were used as numerals, this was not the case with the Roman letters and there was a great variety of opinion as to their numerical value.

In the Latin translations the Sphere is attributed sometimes to Democritus, sometimes to other authors such as Pythagoras, Apuleius, Plato and others.

In 1914 E. Wickersheimer in a very interesting study entitled *Figures médico-astrologiques des IXe, Xe et XIe siècles*[5] published various versions of the Sphere from the following manuscripts:

Bibl. Nat. nouv. acq. lat. 1616, saec. IX, fol. 7^{v}
Bibl. Nat. lat. 8663, saec. XI, fol. 57^{v}
Orléans 276, saec. XI, p. 129

In the same paper Wickersheimer published two versions of the so-called *Tetragonus subjectus*, a divination device very similar to the Sphere. He published them from the manuscripts:

Bibl. Nat. nouv. acq. lat. 1616, saec. IX, fol. 14
Laon 407, saec. IX, fol. 136^{v}

Charles Singer in his essay *Early English magic and medicine*[6] published the Sphere from

Bibl. Nat. lat. 11411, saec. IX, fol. 99
Bodl. 718, saec. X, fol. 50^{v}-51^{r}

Lynn Thorndike in *A History of Magic and Experimental Science*

[4] Nechepsonis et Petosiridis fragmenta magica, *Philologus*, Suppl. VI, 1891-93, pp. 325-394. Facsimiles of the Sphere in M. Berthelot, *Collection des anciens alchimistes Grecs*, vol. I, 1887, pp. 88-90 and A. Bouché-Leclercq, *L'Astrologie Grecque*, Paris, 1899, pp. 539-540.

[5] *Janus*, vol. 19, pp. 157-177.

[6] *From Magic to Science*, New York, 1928, pp. 133-167.

gives a list of manuscripts that contain the Sphere and similar texts.[7]

In the following I would like to publish three versions of the Sphere and some additional texts from three manuscripts that are not included in Thorndike's list.

2.

ST. GALL, STIFTSBIBLIOTHEK, CODEX 752, SAEC. X, p. 82 (Fig. 1).

The text reads:

Spera apulei platonici, de vita, de morte, vel omnibus negotiis, et quicquid inquirere volueris. Sic computabis per omnes litteras ut puta de nomine aegri, addes et lunam quota fuerit die qua decubuit. Et quotiens potueris, XXXmam deduces et quod superaverit, reverteris ad organiolum infra scriptum. Et si superiori convenerit parti numerus, vitalis erit. Si inferiori parti, moriturum dicia. Sic et de omnibus negotiis vel causis requiris.

At the right is the Roman alphabet with the numerical value of each letter. Over the Sphere to the left is the Greek word ΥΠΕΡΓΕΙΟΝ which designates the superior hemicycle.

The numbers in every hemicycle of the Sphere are arranged in three groups. This is not by accident. In the more elaborate Sphere of Petosiris[8] the three sections of the hemicycle of life from left to right are designated as *μεγάλη ζωή, μέση ζωή, μικρὰ ζωή*. This meant that if the number was found in the left upper section, the recovery would be rapid, but slower if it occurred in the central section and slowest if in the right-hand section.

Correspondingly, the three sections of the hemicycle of death were designated as *μέγας θάνατος, μέσος θάνατος, μικρὸς θάνατος*, and the interpretation followed the same principle.

In the simplified mediaeval Latin sphere the prognosis was not differentiated that far. It was simply life or death, but the traditional grouping of numbers remained.

In this manuscript the Sphere is attributed to Apuleius Platonicus. The philosopher and novelist, Apuleius of Madaura (2nd century A. D.), enjoyed in the Middle Ages the reputation of having been a great magician. This was probably due to the fact that he was

[7] Vol. I, New York, 1923, pp. 692-694. See also Thorndike's *A Catalogue of Incipits of Mediaeval Scientific Writings in Latin*, Cambridge, Mass., 1937.

[8] Bouché-Leclercq, *l. c.*

Spera apulei platonici · decuria · demorte · t omnib negotiis
eque quid inquirere uolueris · Sic compitabis per omnes
litteras utputa denomine aegri · adde & lunā quota fue-
rit die qua decubuit · t quotiens poteris · xxx mā deducas
& qđ superauerit · reuertaris adorganiolū infra scriptū · t ubi
superiori conuenerit pusta numerus · uitalis erit · Si inferiori
pusta · mortuarii dies · Sic eadem omnib negotiis t causis requiris ·

THYMIAMA PALTGRIMI·

Gozumbri — III· Gumfiu · — I·

Aloa den VI· Mustaden II· Cafforo den II·

Hec omnia terendo misceantur — ' ſ

A·	III
B·	XXVII
C·	XV
D·	XXII
E·	XII
F·	XIII
G·	XV
H·	IIII
I·	XII
K·	VIII
L	VI
M·	III
N	XVIII
O·	III
P·	XVIII
Q·	XV
R	XVIII
S	VIII
T·	XI
V·	VII
X·	VI
Y·	II
Z·	I

Fig. 1.

SPHERE OF APULEIUS

Manuscript St. Gall 752, 10th century, p. 82.

once actually accused of having bewitched his wife Aemilia Pudentilla, an accusation against which he defended himself in his still preserved *Apologia*. It is needless to say that the attribution of the Sphere to him is purely fictitious.

3.

The page of the St. Gall manuscript that contains the Sphere has at the bottom an interesting recipe:

THYMIAMA PALTGRIMI

Cozumbri—iii. Cumfiti—i. Aloa den. vi. Musci den. ii. Caffore den. ii. Haec omnia terendo miscentur.

It is a *θυμίαμα*, a kind of incense. Such *θυμιάματα* were used in the cult[9] but also for medical fumigations. As to the ingredients, cozumber (coczumber, cotzumber) was a precious kind of aromatic gum that was used as incense in the church,[10] and also in salves and plasters.[11] Cumfitum occurs in other prescriptions as cumfita, gumfita, usually in connection with gums.[12] I am not sure what it actually is. Aloe was frequently used in ancient recipes. Muscus is here undoubtedly musk, and caffora is camphor which was imported from the East. Camphor occurs in early mediaeval recipes as cafora, casfora,[13] and St. Hildegard mentions it as gamphora.[14]

The prescription is particularly interesting because it has the name of its author Paltgrim attached to it. This is a Germanic name, not that of an ancient author, and it is quite possible that Paltgrim was a St. Gall monk.

A similar thymiama, but with more ingredients, occurs in another St. Gall manuscript, codex 44, saec. IX, p. 247:[15]

Confectio timiame

Cozumbrio lib. i, storace unc. ii et den. ii, confiti unc. iii et den. vi, thus unc. i den. ii, mirra den. vi, mastice unc. 8, spica unc. i et den. vi, croco den. ii, aloen unc. i et den. vi, cafora unc. i et den. i, musco den. iiii, ambar den. i.

[9] Herodotus, I, 198.

[10] Ducange, *Glossarium mediae et infirmae latinitatis*, vol. II, 1883, p. 386.

[11] H. E. Sigerist, *Studien und Texte zur frühmittelalterlichen Rezeptliteratur*, Leipzig, 1923, pp. 21, 61, 89.

[12] *Ibid.*, pp. 49, 55, 61, 62, 64, 89. [13] *Ibid.*, pp. 89, 147.

[14] *Causae et curae*, ed. P. Kaiser, Leipzig, 1903, pp. 215, 26.

[15] H. E. Sigerist, *l. c.*, p. 89.

There is one last difficulty on our page. After the recipe there is a sign that looks like an S on top of a V. What this means I do not know. It may be an unusual abbreviation for salva venia, solvi votum, or something similar.

4.

VERCELLI, ARCHIVIO CAPITOLARE, CODEX CLXXVII (Arab. 42), SAEC. X, fol. 143^{r} (Fig. 2).

The text reads:

Ratio sperae phitagorae phylosophi quam apulegius descripsit, ut de quacumque re scire vel consulere volueris, utpote de aegris, qua die incurrit, et ea die quota luna fuerit scire debes, addis nomen ipsius hominis secundum infra scriptas literas, et sic in unum colligis et partire in triginta et quicquid remanserit respicies, et sic invenies, et si sursum inveneris, vivet et prosperum erit. Si autem infra fuerit morietur vel contrarium erit.

In this manuscript the Sphere is attributed to Pythagoras, and Apuleius is supposed to have copied it from him. The text is not identical with that of the St. Gall MS. but the content is the same. The beginning of the text is very close to that of MS. Bibl. Nat. nouv. acq. lat. 1616 published by Wickersheimer.

The Sphere is arranged somewhat differently from that of St. Gall. The numbers also occur in groups of three but they are not the same:

Lucky numbers

St. Gall: 1 2 4 8 11 13 14 16 17 18 19 20 22 23 27
Vercelli: 1 2 3 4 6 7 10 11 13 14 16 17 19 20 22 23 26 27

Unlucky numbers

St. Gall: 3 5 6 7 9 12 15 19 21 24 25 26 29 30
Vercelli: 5 8 9 12 15 18 21 24 25 28 29 30

The numbers 10 and 18 do not occur at all in the St. Gall Sphere, while 12 and 15 occur twice, and 19 also occurs twice, once on the lower and once on the upper half of the Sphere which must have been rather confusing. It shows that the St. Gall Sphere is somewhat corrupt.

The Vercelli Sphere has on the upper hemicycle the words DE CAELO VITA + DE AUSTRO CALOR, on the lower hemicycle DE TERRA MORS + DE AQUILONE FRIGUS.

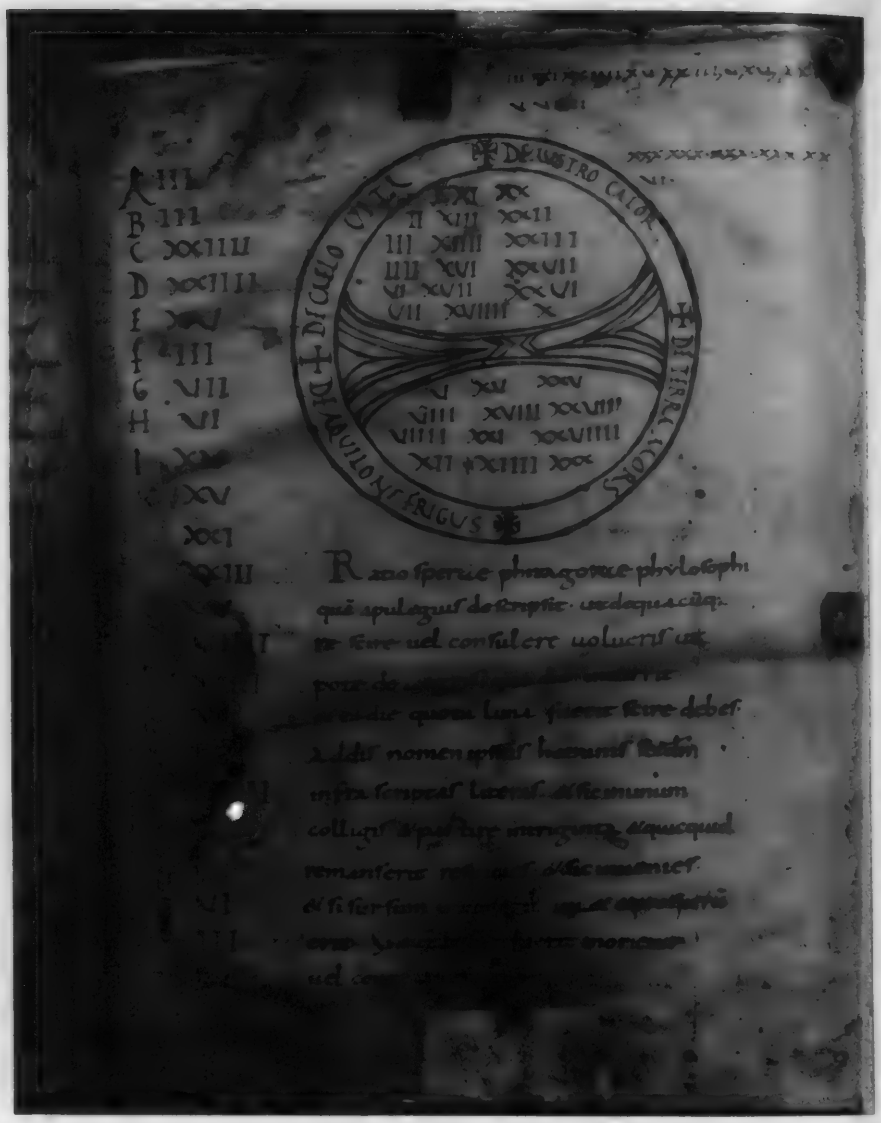

Fig. 2.

SPHERE OF PYTHAGORAS

Manuscript Vercelli, Archivio Capitolare CLXXVII
(Arab. 42), 10th century, fol. 143^{v}.

A very interesting item is found in the right upper corner. Here someone, perhaps the scribe himself, perhaps someone else, consulted the sphere. The first two lines read:

III XXI XXIIII XV XXIII V XV XXI
V VIIII

This in all probability is the numerical value of the person's name. The value of the individual letters is given along the left margin, namely

A III B III C XXIIII D XXIIII E XV F III G VII
H VI I XV K XV L XXI M XXIII N XV O VIIII
P XIIII Q XXI R XIII S VIIII T VIII V V X VI
Y III Z III

Thus we should be able to find the man's name. We have the following possibilities:

III	A B F Y Z
XXI	L Q
XXIIII	C D
VX	E I K N
XXIII	M
V	V
XV	E I K N
XXI	L Q
V	V
VIIII	O S

The most probable choice of names we obtain is: [16]

Alcemuelus	Aldemuelus
Alcimuelus	Aldimuelus
Alcemuilus	Aldemuilus
Alcimuilus	Aldimuilus

[16] My colleague, Prof. Leo Spitzer of the Romance Language Department of The Johns Hopkins University, tells me that such a name as Aldemuelus is possible. It is Germanic and would correspond to " Altmut " or " Edelmut." See Albert Heintze, *Die Deutschen Familiennamen*, 5th ed., Halle, 1922 and A. Longnon, *Polyptique de l'Abbaye de St. Germain-des-Prés*, Paris, 1895, p. 277 where such names as Adalmodus occur.

Fig. 3.

Sphere of Life and Death
(Fragment)

Manuscript Florence, Laur. plut. 73, cod. 23, 13th century.

The next figures we find in the right upper corner are:

XXX · XXX · XXX · XXX · XX
VI ·

Our man apparently divided his name by 30 after having added the day of the moon to it, and he was lucky enough to obtain a remainder of XXVI.

I suspect that VI was the day of the moon and that it was written on a special line for that reason. There was space enough to have it follow the XX. The sum total of the name is 141. Four times 30 plus 20 is 140. So a mistake must have been made somewhere, but even with a remainder of 27 our man would have been on the lucky side.

5.

The verso of folio 143 of the Vercelli manuscript has an interesting prescription for the treatment of people possessed by a demon which deserves to be published. The leaf unfortunately is heavily stained and the script has faded in many places. The text reads:

Ad homines demoniosus . . . AOYN . . . OYC denarios duos et dimidio, herba sub ti . . . a colliges cum oratione dominica et viscu de collore aut de cerro cum aqua sancta colotio capre virginis dabis luna ΔEK . . . Prius tamen ut missas superfiant cantatas et sedeat ipse homo sub altario diebus VIIII postea dato et . . . diat hanc penitentia annum plenum custodiat vir si fuerit a muliere et mulier a viro cum vitro non bibat et carne caprina non comedat dum vivit nec lente nec carne porcina nec anguila nec barbium pisce neque tenca neque caput ullius animalis neque de pisce panis de dominico die factum non comedat tandem si vir fuerit colotio capri masculi si femina femine virginis sint et parvi hoc autem si custodierit liberabitur si autem preterierit statim revocetur in pristinum locum. Et ibi ter dicit extinguo c p n c f p c k p n k s h x k x s m x l k f r k s b b p m n k c p k t x x k r k l k x k . . vel ex hac hora quam voluerit salus . . . ita recte suis temporibus purgetur. Deinde ipse statim in buxide recondis qua volueris claud . . . ipsa buxide in linteolo involvis diligenter ligas et signas et absconde ea ut non aperiatur ita ut illa nec sol nec . . . aliquando videat aliquando . . . et iterum co . . . reddis.

The text is so corrupt that I cannot give a literal translation. The treatment apparently consisted of the following procedure:

For nine days masses were sung in the church while the patient had to sit under the altar. Then on the tenth day of the moon he was given a potion containing 2½ drachms of a plant that had been

	Vercelli	St. Gall	Flor.	B. N. 11411	B. N. 1616	B. N. 8663	Orl. 276	Bod. 718	B.N.1616 Wick., p. 172
A	3	3	3	3	3	3	3	3	3
B	3	27	6	3	3	3	3	3	3
C	24	15	26	22	27	26	22	28	15
D	24	22	25	23	26	23	24	24	23
E	15	12	16	12	15	17	15	25	15
F	3	21	3	3	3	3	3	3	3
G	7	15	21	7	7	21	7	7	7
H	6	4	18	6	3	29	6	6	6
I	15	12	11	15	15	12	15	15	15
K	15	9	26	15	15	26	15	16	15
L	21	6	21	21	21	21	25	21	20
M	23	3	?	22	23	17	24	23	25
N	15	18	11	16	15	15	15	15	15
O	9	3	9	9	8	8	8	9	9
P	14	18	12	24	17	12	?	14	14
Q	21	15	9	21	21	9	21	21	30
R	13	18	13	13	13	13	12	12	13
S	9	9	14	9	9	18	9	9	9
T	8	11	6	8	9	6	9	9	8
V	5	7	7	5	6	7	6	16	5
X	6	6	12	6	6	13	15	10	6
Y	3	2	2	3	3	2	6	6	3
Z	3	1	3	3	2	3	3	3	3

Fig. 4.

Numerical value of letters of the Roman alphabet.

gathered while the Lord's prayer was said. The Greek name of the plant was given but it is so mutilated that I cannot identify it. The potion contained in addition mistletoe from an oak (de cerro) or from another tree (de collore) [17] and holy water. The whole was given with urine of a virgin goat. As a penance the victim had to abstain for a whole year from sexual intercourse. Throughout his life he was not to drink from a glass, nor was he to eat goat meat, lentils, three kinds of fish (eel, barbel, tench), the head of any animal or fish, bread baked on a Sunday. The following is not quite clear because the text is corrupt. The victim had to do something, perhaps wash himself with urine of a male or female virgin goat according to the demoniac's sex. Then he had to pronounce a charm three times beginning with the words "I extinguish." The charm consisted of a series of letters, all consonants, that do not make any sense. If the whole ritual was followed in the right order and all taboos were observed, the demoniac was liberated. Otherwise he relapsed into his former condition.

The end is like the beginning, an instruction for the priest. He shall put the charm back into a box, close it, wrap it into a small piece of linen, bind it, sign it and conceal it so well that neither sun nor moon will ever see it.

6.

The third Sphere is a mere fragment. It is found on the recto of the first leaf which is part of the binding of the *Florence Manuscript Laur. pluteus 73, codex 23, saec. XIII* (Fig. 3). It gives the numerical value of the letters of the Roman alphabet, but the scribe never finished his job.

7.

As we mentioned before, the numerical value of the Greek letters was given, since they all served as numerals. This was not the case with the Roman letters. It is very striking that not two manuscripts of the Spheres published so far have identical numbers (Fig. 4). They are unanimous in assigning the number 3 to the letter A. Eight out of nine manuscripts have 3 for F, seven have 3 for B and Z, but otherwise there is considerable divergence.

[17] About the magic virtues of mistletoe, see Leo Kanner, Mistletoe, Magic and Medicine, *Bulletin of the History of Medicine*, 1939, vol. VII, pp. 875-936.

A TRANSLATION OF OLOF RUDBECK'S *NOVA EXCERCITATIO ANATOMICA* *

ANNOUNCING THE DISCOVERY OF THE LYMPHATICS (1653)

TRANSLATED INTO ENGLISH BY

AAGE E. NIELSEN, M. D.**

With a biographical note by

Professor GÖRAN LILJESTRAND, Stockholm.

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BIOGRAPHICAL NOTE †

Olof (Latin *Olaus*) Rudbeck, the son of Bishop Johan Rudbeck, was born toward the end of the year 1630, probably on the 11th of December in the small town of Vasterås. His father, a personality of outstanding cultivation in Sweden, gave him a thorough education. Olof went to the University of Upsala in 1648 where he took up medicine and botany. His important discoveries roused the attention of Queen Christina who made it possible for him to go to Leyden. On his return to Upsala he became first assistant in medicine, then associate professor, and finally in 1660 professor in the medical faculty, but he also taught chemistry, astronomy, mathematics, architecture and music. In 1661 at the age of 31 he was made Rector of the University, and from that time until the end of his life he con-

* The English translation was made from the Swedish rendering by Robert Tigerstedt, "Ny anatomisk undersökning om leverns vattenklara utförsgångar och körtlarnas serösa kärl," in *Dokument ur medicinens historia under sextonde och sjuttonde århundradet*, Stockholm, Albert Bonnier, 1921 (pp. 363-403). Through the kindness of Dr. W. W. Francis of the Osler Library, McGill University, Dr. Nielsen's English version has been closely compared with the Latin text and corrected in the light of it.—J. F. F.

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† Translated from the German by Miss Dorrit Willner.

tinued his active and fruitful practical work for the university and the whole country. Besides this he found time for scientific problems, but he was primarily interested in botany, a domain in which he is regarded as Linné's predecessor. Between 6,000 and 7,000 illustrations were made for an extensive botanical work, only a small part of which was printed (*Campus Elysii*). Later on he concentrated on a great work, *Atlantica*, concerning the ancient history of Sweden in which he tries to prove the antiquity of Swedish culture. This work contains several brilliant and valuable observations, as well as much that is fantastic and uncritical.

But Rudbeck had accomplished his most important scientific work while still a young man. It was concerned with the lymphatic system and thus lies on the borderland between anatomy and physiology, a domain that was making special progress at that time. In the years 1611-13, while still professor at Upsala, Bishop Rudbeck had given the first lectures on anatomy in Sweden, and is said also to have been the one who first interested his son in the subject. The latter began to read the works of Harvey, Colombo, Fabrizio and others with enthusiasm. The fruit of his own work appeared not long thereafter. But before taking up this matter in detail, a short digression into the past is necessary.

Herophilus and Erasistratus were supposed to have seen lacteal vessels although this fact had been forgotten. They were rediscovered by Gasparo Aselli of Cremona who on July 23, 1622, made an experiment with a dog to demonstrate to his friends the movements of the diaphragm. After the opening of the abdomen numerous white cords appeared in the mesentery. At first Aselli took them for nerves but soon found that a milk-like fluid welled up when he made a small incision in these cords with his knife. When the dog subsequently died during this experiment the white cords emptied out so that they were barely visible. Aselli repeated the experiment, at first without success, but later found the white cords again after he had fed the animal shortly before the incision. He named them *venae albae et lacteae* (from *lactis*, intestine, not *lac*, milk) and followed their course to a glandular mass lying on the edge of the mesentery in the dog which he called pancreas (later *pancreas Aselli* or *p. glandulosum*). From this gland he followed the vessels further to the orifice of the

liver and found that they now contained a pale fluid. Besides the lacteal vessels Aselli had undoubtedly also seen and portrayed the lymph vessels of the liver, but he took them for a direct continuation of the former and thought that they carried the chymes from the gut further beyond the liver. Without any doubt he was influenced in this assumption by the belief—then commonly accepted—that the blood was produced in the liver. Six years later lacteal vessels were demonstrated in a human being (in a criminal who had consumed a large meal before his execution).

The next great step forward was made by Jean Pecquet, a young medical man of Montpellier. He observed after removing the heart of a living dog that a milk-like fluid streamed out of the superior vena cava. Pressure on the abdomen caused renewed overflow of the fluid. In a subsequent experiment he saw a vessel resembling the chyles vessels in the thoracic cavity. As he tied a ligature around the vessel it swelled in its lower part; with loosening of the ligature the fluid re-appeared in the superior vena cava. Pecquet was also successful in finding the orifice of the duct (*ductus thoracicus*) in the subclavian vein and further noted that the duct or ducts—he generally saw two—entered a dilatation (*cisterna chyli*) underneath the diaphragm. In his paper published in 1651 Pecquet drew the conclusion that the production of blood could not take place in the liver. But at the same time he gave utterance to the opinion that the vessels passed from the cisterna to the kidneys where the chyles might be secreted.

Rudbeck himself reported the course of his investigation in the paper which follows. In 1650 he happened to be present when a calf was butchered and in it he saw a whey-like fluid near the supraclavicular notch—closely resembling that of Pecquet. At closer inspection a bladder appeared near the abdominal aorta filled with the same fluid and Rudbeck named it *vesicula chyli*, or milk bladder. The orifice of the duct had been torn during the butchering. In the following year Rudbeck demonstrated in a cat the flow of the fluid from the lacteal vessels to the milk bladder and the thoracic duct as well as the discharge of the latter at the junction of the subclavian and the jugular. It was not until April 1652, when Rudbeck was permitted to make the famous demonstration of his findings before Queen Christina, that the assembled court-physicians informed

him of Pecquet's work on the thoracic duct which had just appeared in print, and shortly thereafter he also read of its further proof in a human being by J. van Horne and Thomas Bartholin. By this time Rudbeck's work had attained quite different dimensions. One discovery followed the other. In the years 1650 and 1651 he observed the lymph vessels of the liver—demonstrating them also before the Queen—and in 1652 he showed that the fluid does not flow towards the liver. Early in 1651 he studied the lymph vessels of the rectum and followed them to the cisterna chyli; in the same year also he traced lymph vessels to an esophageal gland. To these he added in the following years lymph vessels that lead from the gland alongside the crural vein to the cisterna, and also established the existence of such vessels in the abdominal wall and of others that lead into the thoracic duct from two glands situated between lung and heart. In 1653 he came upon lymph vessels on the posterior side of the sternum.

The demonstrations before the Queen are the best indication of the great interest aroused by Rudbeck's observations. Nevertheless it was not until the summer of 1653 that a communication concerning his discoveries appeared in print. However, in May 1652 he had published a dissertation entitled "De circulatione sanguinis" partly with the purpose of introducing the new teaching of Harvey into Sweden, and partly to deal with the question, raised at the end of the essay, of the origin of the blood in the liver, which Rudbeck denied. This proves that he had freed himself from the common belief and as a result of his own experiments had arrived at the same conclusions as Pecquet. The main work, printed in Västerås, appeared with the title *Nova excercitatio anatomica, exhibens ductus hepaticos aquosos et vasa glandularum serosa*, a title suggesting Harvey's influence. The publication comprises 48 pages in small quarto, including a dedication to the Swedish chancellor Axel Oxenstierna, a number of miscellaneous anatomical observations and a letter of congratulation from his brother, as well as two excellent illustrations drawn by Rudbeck himself.

Rudbeck deals with the transparent excretory ducts of the liver and the lymph vessels of the remaining body, each separately. This might be explained by the special part the vessels of the liver had previously been believed to play, but Rudbeck emphasizes that the two kinds of vessels coincide in every respect.

The first sentence of the text indicates his method of proof. It appears that he detected the course of the fluid in the vessels by placing ligatures in the manner described by Harvey to prove the direction of blood circulation and by Pecquet to prove the course in the thoracic duct. At first Rudbeck thought that the vessels he had discovered contained a special fluid secreted by the pancreas and its excretory duct which had been discovered shortly before and also taken for a chyles vessel (by de Back). Later on he confirmed the fact that the vessels proceed from the liver to a small gland and further on to the cisterna; for the chylous vessels to the junction with the milk bladder could readily be demonstrated. While delicate lacteal vessels lead to the pancreas Aselli, a smaller number of coarser trunks carry on from the gland. Rudbeck used ligatures systematically to find the course of the new vessels in the different parts of the body. He minutely described them as well as their relation to the glands and emphasized that they more or less permeate all parts of the body. The most important findings have already been mentioned. Rudbeck depicted the walls of the vessels as membranous. He regularly met valves by means of which alternate constriction and dilatation of the vessels were brought about. The presence of the valves also explains why he did not succeed in probing the vessels and in blowing them up with air (but Bartholin inflated them proximally from the incision).

Rudbeck also discussed the significance of these newly discovered vessels in the body. Obviously the liver could not play a central part in the production of the blood from chymes, but this already seemed clear to him from van Horne's and Bartholin's publications on the cisterna. Personally he seemed to imagine a kind of sweating out of the blood but did not dare utter a definite view regarding the importance of the matter, an attitude which is not astonishing if one considers how rudimentary the science of metabolism was. Finally he connected the formation of ascites with stasis of the lymph.

Rudbeck's notes are based on numerous experiments; he himself stated later on that he used almost 400 animals (cat, dog, calf, sheep, goat and wolf).

In some later instances Rudbeck made new and positive contributions to the problem of the lymph vessels and their extent. While in

Leyden in 1653 he saw the lymph vessels of the anterior extremities which had previously been described by Bartholin, and in the succeeding years those of the stomach, spleen, testicles and uterus; he also saw lymph vessels from the liver going directly through the ligamentum suspensorium hepatis to the thoracic duct. Illustrations of these new findings were given in the second edition of the *Excercitatio* (in Hemsterhuis, *Messis aurea,* 1659). He also mentioned (1654) that the contents of the lymph vessels had a salty taste and coagulated easily. Finally in 1661 a dissertation was presented under the presidency of Rudbeck, *De sero ejusque vasis* ‡ by O. Figrelius in which the lymph vessels of the tongue, the diaphragm and the intercostal spaces are described.

The question of priority in the discovery of the lymph vessels has been discussed thoroughly from many angles. That Aselli saw the lymph vessels long before Rudbeck, but ascribed a wrong function to them, is certain and has often been emphasized. Similar observations have also been mentioned, viz., by Eustachio, Falloppio, etc. It is more important that some investigators before Rudbeck had encountered lymph vessels and clearly recognized their course. This is said to be true for the celebrated iatro-mechanicist Sylvius de le Boë who in 1660 stated that he had seen such vessels for twenty years without publishing anything about them. The anatomist Glisson mentioned in 1654 that a young English student, George Joyliffe, as early as 1650 had observed the vessels concerned in almost all parts of the body. Because of his early death (1652) his results had never been published.

These observations can obviously be no serious threat to Rudbeck's priority. More important is the dispute with the learned Danish Professor, Thomas Bartholin, which caused a sensation. The latter's main publication appeared in 1653, that is, shortly before Rudbeck's, but he had already described the lymph vessels in a paper in 1652, although postponing his opinion about them. Two questions take the foreground in the discussion: (i) the discovery of the lymph vessels of the liver and with it the part this organ fulfills in the production of blood on the one hand, and the proof that in general

‡ Printed in Haller's *Disputationes anatomicae selectae*, Vol. 7, 1751 (pp. 235-248).

lymph vessels are present in the different parts of the body, on the other. The extensive explanations about this—especially the representations of Sommer, Tigerstedt and Gosch—seem to me to show clearly that both scientists came to the correct understanding of the two questions independently. While Rudbeck's observations were made earlier, Bartholin had priority of publication, if the demonstration before the Queen Christina is not to be regarded as such. Therefore one can agree with Tigerstedt when he says that both scientists should be called discoverers of the lymph vessels with equal right—or perhaps more accurately discoverers of the lymphatic system.§

§ The rather extensive literature, chiefly written in the Scandinavian languages, has recently been arranged by O. T. Hult, *Hygiae*, 1931, *93*: 317. Other non-Scandinavian publications referring to this are: Daremberg, Ch., *Histoires des sciences médicales*, Paris, 1870 (see vol. 2, p. 638); His, W., Ueber die Entdeckung des Lymphsystems. *Z. Anat.*, 1875, 1: 128; Fulton, J. F., The early history of the lymphatics. With particular reference to Bartholin, Rudbeck and Joyliffe. *Bull. Hennepin Co., med. Soc.*, Minneapolis, 1938, *9*, 5-10.

NEW ANATOMICAL INVESTIGATION

DEMONSTRATING THE AQUEOUS EFFERENT DUCTS OF THE LIVER AND THE SEROUS VESSELS OF THE GLANDS,

NOW FOR THE FIRST TIME DISCOVERED AND DESCRIBED WITH ILLUSTRATIONS

BY OLOF RUDBECK, SWEDE *

* The illustrations, the initials and the vignettes are taken from the original edition of *Nova excercitatio anatomica, exhibens ductus hepaticos aquosos, et vasa glandularum serosa, nunc primum inventa, aeneisque figuris delineata.* Västerås, E. Lauringer, 1653.

DEDICATORY EPISTLE*

TO THE CHANCELLOR AND MOST WORTHY SENATOR OF HER SACRED MAJESTY THE QUEEN AND OF THE KINGDOM OF SWEDEN, PROVINCIAL JUDGE OF THE PROVINCE OF WEST BOTHNIA, AND MAGNIFICANT CHANCELLOR OF THE UNIVERSITY OF UPSALA, THE ILLUSTRIOUS AND EXCELLENT LEADER AND LORD, LORD AXEL OXENSTIERNA, COUNT OF GÖDERMORE, FREE BARON OF KIMITHUS, LORD OF FÝHOLM AND TYDÖÖN, KNIGHT, AND THE AUTHOR'S LORD AND CLEMENT PATRON.

* * * * * * *

Most illustrious Leader:

After giving my attention to the study of medicine, I thought my first task should be the acquisition, so far as possible, of an accurate understanding of the human body. This is indeed a very difficult but necessary matter. For that reason Herodotus says it was customary among the Egyptians for men to be physicians of single parts of the body only, evidently because they believed the life-span and intellect of one man insufficient for the thorough study of the whole body. Be that as it may, diligent observation, frequent dissection, and a certain inner force of nature, with the divine blessing, make all things easy and obvious. With such has it occurred that I (far be arrogance from my words!), though I may not have progressed beyond others, have yet found some things hidden from them. These I now make public.

For I have noted that the office of the liver is not alone to separate black and yellow bile from the blood but also to pour a watery humor through certain hepatic ducts, never recognized by the other teachers of this art, into a chyle-bladder, for the purpose of fermentation or of dilution as I believe. Nor can I consider a knowledge of these ducts to be useless in medicine; The obstruction of the ducts proves the contrary. For this obstruction causes those vesicles of the liver known to all doctors which are the cause of abdominal dropsy. It is moreover accepted by the judgment of many anatomists and physicians that the glands of the body draw out from the blood

* Translated by Gerard Fountain, Jr., Yale University School of Medicine.

a serous fluid like a residue; by what means this is done, however, and by what channels they then rid themselves of the fluid, was obscure: this fortune has revealed to me. For glands situated near the crural veins, the sides of the heart, the esophagus, and the mammary veins, carry a water liquid to the chyle ducts by virtue of certain vessels.

Though these academic studies are not, I think, worthy to be inscribed to so great a Name; yet they dare to invoke the splendor and patronage of that Name which not only understands and loves studies of every kind but also cherishes the endeavors of the active intellect. It happened that not long ago, with unparalleled humanity, you rewarded my meagre studies by examining and following them. May these too be likewise distinguished, as proof at least of your liberal favor to me, of which I have not failed to make use.

Then may God the Best and Greatest who has lavished upon you a zeal and industry to deserve better and better of your country and the republic of letters give you strength in abundance in this your old age, and prolong it.

One most devoted

To your illustrious name,

Olof Rudbeck.

PREFACE

This century is fruitful in criticism and misrepresentation; so it occurred to me, when about to publish these new anatomical observations, that there would be many to judge my unpretentious work with disapproval and scorn. Even if they knew nothing else of the famous comic writer, they would quote his "Nothing is said that has not already been said." The divine Hippocrates, they would say, is the dictator and high priest of our art; and did he not derive knowledge from Aesculapius, his supposed ancestor, according to Soranus, as well as from his father Heraclitus, the orator Gorgias of Leontium and the philosopher Democritus of Abdera, whose pupil he was? And later, did not Galen of Pergamus receive his knowledge from Hippocrates and, after arranging it in an incomparable system, communicate it to others? And did not the Arabians, Avicenna, Aegineta, Averrhoes, Mesue and many other famous men, follow his example and walk in his footsteps?

Although I well know that experienced physicians share my opinion, I shall briefly answer those questions. I readily admit that practically nothing can be said or written by the dogmatists that cannot easily be found in their previous writings. But meanwhile, since several eyes see more than one eye, and new discoveries do add something new to our knowledge, many others have observed new facts and have taken a different but far from backward path which often proved to be both easier and shorter.

I shall mention one example amongst many. In the therapeutics of our modern medicine, what could possibly have brought about greater improvement than our chemical preparations? Indeed, no matter how prejudiced and timid we may be, swearing by the ancients even to the point of being their lick-spittles, we should not in our time be willing to do without the many select preparations, drugs and prescriptions discovered by the chemists—their essences, extract, salts, powders, spirits, quintessences, tinctures, balsams, elixirs, etc. And that the medicine of Hippocrates and Galen in its several parts has been advanced through these means in general, and each and every one in particular, no man of good sense or good will can deny.

But to come closer to our subject, anatomy has such breadth and depth that all must acknowledge its importance; and with all due respect to that African comedian,* I think it completely refutes his axiom which ought to be not merely changed but reversed: "Much has been said and much remains to be said which has not been said before." For who had any knowledge of the valves of the veins before Colombo, of the uterine tubes before Falloppio, of the duct of the pancreas before Wirsung, of the lacteals of the mesentery before Aselli, or of the circulation of the blood before Harvey?

Nor is it rash to insist that the Almighty, wonderful in all His works, has not granted all His gifts and blessings to any one century. On the contrary one day teaches, so to speak, the next, and it is quite clear to many that even in the weak and decrepit old age of this world of ours some great new works of genius may aspire to the light of which an earlier age, otherwise diligent enough, was completely ignorant.

But just as no reasonable person would brazenly insist that the

* P. Terentius *Afer*.

new in itself is preferable to the old, so am I in agreement only with those who do not accept and cling to the old to the extent of despising the new and repudiating it without a trial. For my part, I would have all sciences reach such perfection as to leave nothing to be discovered, but failing that, each period should add something new to them. Criticism will be welcome to me from those who are experienced in science since, even if not laudatory, it will add to my knowledge.

Chapter I

The discovery of these new vessels.

During the years 1650 and 1651 while engaged in studying the origin and course of the lacteal veins, when I placed a ligature around the portal vein and the bile duct, sometimes ducts became visible, distinctly gorged between the liver and the ligature, but collapsed below the latter. That these ducts could have been chylous vessels was inconceivable to me, because when the ligature was released, they collapsed, the watery fluid they contained rushed toward the pancreas and they disappeared; but again became filled with fluid when the ligature was tightened. Having repeated this experiment several times and convinced myself of its correctness, it occurred to me that the fluid in question was, so to speak, useless for the sustenance of the body and was excreted in the pancreas and driven out through Wirsung's duct. I was so sure of my point, that during the public demonstration of the thoracic lacteals which I once arranged in the presence of Europe's great lady, our illustrious and gracious Queen Christina, and of the nobility, the honorable senate of the university and a number of students, I also demonstrated these ducts.

When, however, somewhat later I tied the lacteal veins of the mesentery between the pancreas glandulosum and the chyle vesicle so as to make apparent in the course of a few days the lacteal fluid contained in them, by good luck both the origin and the orifice of these ducts, about which I thus far had been uncertain, became perfectly clear. Concerning this I shall now give a fuller account in accordance with the plan of my work.

Chapter II

The name, nature and origin of these vessels.

To avoid what generally befalls the seafarers who forget the name of their harbour and expose themselves to the winds without knowing where they are being carried, the first thing to do, after the manner of the greatest anatomists, is to give a name to these new structures, so that after a few repetitions no doubt remains as to what is meant. I accordingly distinguish these vessels by the name of aqueous hepatic ducts (*Ductus hepatici aquosi*); hepatic, because the fluid contained within comes from the liver where they take their origin; and aqueous, because their lumen is filled with a fluid of that nature.

The *substance* of these ducts, as far as one can see, is membranous, and their extremely thin wall cannot be separated with the knife into layers, since in most instances it does not even tolerate its edge. Thus substantially these ducts are not at all unlike the lacteal vessels.

Their *temperament* probably is cold and dry inasmuch as they consist of membranes, and membranes, as all physicians, anatomists and philosophers consider to be definitely established, have received in their substance the coarser part of the sperm. This idea, however, is in contrast to that of the most careful anatomists of this century who, on the basis of their own observations, definitely maintain that the seed of neither man nor woman materially affects the production of the foetus.

These ducts have their *origin* in the liver (Tab. I. *eeee*). However I have not succeeded in determining definitely if they originate in the parenchyma of the liver by a ramified system of small branches, not forming larger branches until outside the liver. Because they avoid, as it were, the edge of the knife, and it is impossible to follow their course towards the liver, either by the passage of a sound or by blowing in air, owing to their numerous valves which open in an outward direction. These ducts leave the liver high up as well as further down, running alongside the portal vein (Tab. I., *O*) and bile duct (Tab. I., *gg, h*) and in addition coursing downward (Tab. I., *fff*) in front of the vena cava in the vicinity of the pancreas carnosum (Tab. I., *bb*), which lies close to the duodenum and stomach,

as Nathaniel Highmore[1] has pictured most accurately in his anatomical treatise (Tab. II., *CC*, and Tab. IIII., *AA*. *B*).

Many of these ducts, sometimes all of them, run with ramified small branches to a gland (Tab. I., *m*) and finally empty together with those ducts which pass by the gland, into the chyle vesicle (Tab. I., R.) which is situated between the kidneys posterior to the vena cava and the aorta.

Particularly in the cat one meets with one or at times several such ducts covering the external surface of the gall bladder with small branches and then taking the same course downward as the rest of the hepatic ducts (Tab. I., *nn*).

It is also of great interest to note that, as I have had occasion to observe in three instances, somewhat above the chyle vesicle and below the above-mentioned gland (Tab. I., *m*) there is a distinct connection between the ducts from the liver and two or three lacteal vessels. For after tying off the chyle vesicle there was an increased flow through the ducts from the liver, the downward movement of the fluid was prevented by the ligature, and in the lacteal vessels the stasis could be followed upward as far as the pancreas glandulosum; when the ligature was released the fluid again moved downward filling up the chyle vesicle.

In most, yes, in practically all of the animals which I dissected, I found two, three, four, occasionally several such liver ducts (Tab. I., *rr*) issuing from the liver's greater lobe, which receives the vena cava and is attached to the latter as well as to the left kidney. These ducts surround the lower portion of the vena cava and appear to open into it and for that reason I first considered them to be lacteal vessels. But frequent dissections and investigation of the course of the fluid by means of ligatures convinced me that the ducts in question ended in the chyle vesicle, and it was to the latter that the aqueous fluid was carried. The previously mentioned gland lies below the portal vein and in many animals is firmly attached to it; as a rule only one such gland may be found, occasionally two or three, rarely none at all. This gland is connected with nearly all the ducts from the liver in the same way that the large gland, the pancreas glandulosum, located in the mesentery (Tab. I., *aa*), receives the lacteal vessels from the intestines and subsequently relays them to

the chyle vesicle. The gland swells up markedly with the incoming fluid and if compressed with the fingers it empties in a downward direction.

These ducts are *situated* above and below the portal vein and run from the liver to the chyle vesicle in that fold of the mesentery which joins the liver and the intestine to the back.

Their *number* always varies, two, three, five or ten, sometimes twenty; the less numerous they are, the greater their width, and vice versa.

Their *length* like their *width* also varies with the size of the animal. In the cat, dog and fox they are of about the same thickness as the ureter; in the calf, sheep, goat and wolf, they vary somewhat.

In *appearance* they are round, tubular, and thanks to their valves, remarkably nodular, being constricted at the valves and swollen with fluid between them.

Their own *color* is that of membranes in general. But just as veins are reddish because of the blood, so these ducts reflect the color of their contained fluid.

They are *connected* with the portal vein and the bile duct by the mesentery from which, therefore, they probably derive their membranous walls.

It is hard to say what essential relationship they bear to any part of the body; for according to the anatomists all *intercommunication* is the work of veins, arteries and nerves, even though they cannot be seen. But it may be urged that such communication is assured by the fact that all parts share in nutrition, which is always by way of vessels, even if sometimes invisible ones.

Chapter III

The use of these liver ducts and the function of their fluid.

Everybody knows that nature does nothing in vain, and therefore I believe that these ducts are not formed and arranged purposelessly, although it is very difficult for the human intellect to give any explanation of their function. I have therefore considered it reasonable to leave this question to the judgment of the most learned anatomists and physicians. However, in order not to ignore it entirely, I shall stress some facts for its elucidation.

These ducts of the liver are always filled with a fluid which in living or recently killed animals looks like water, though in the course of an hour or so it takes on a yellowish tint. The reason for this change is obscure.

Not only in living but also in dead animals, even on the second or third day, a constant flow from the liver to the chyle vesicle can be observed. This may best be studied with the ligature.

As regards the limits of this movement, whence and whither, many circumstances are worth considering, among them the function of the liver, since this fluid wells forth there; and also the significance of its mixing with the chyle.

Without exception the ancient and modern physicians, as well as the philosophers, have maintained that the liver is the blood-forming organ and that the process is as follows: in the stomach the ingested food is changed into a gruel-like mass, and the chyle absorbed from the intestines is then carried to the liver by the mesenteric branches of the portal vein which at the same time carry blood to the intestines for their nutrition, as is pointed out by André Du Laurens [3] in his anatomical work (Quæst. 22) in which he very carefully deals with the function of the liver. There would thus be movement in opposite directions of dissimilar substances in the same vessel, even in the minutest, particularly at the points where the capillary vessels surrounding the intestines emerge. Some have held that the chyle and the blood in these vessels move at different times. Others have thought that different branches of the portal vein carry the chyle to the liver and the blood to the intestine, a theory based not on experience but on probability at a time when our knowledge of anatomy was imperfect.

Thirty or more years ago Aselli [5] discovered the structures which carry the chyle. In our time no one disputes their nature and function. But even recent writers think that the goal of these vessels is the liver, although they have never traced them any farther than the pancreas. Bartholin [4] bears this out in his work on the veins (Bk. I, Ch. 3), where he says, " Only our ignorance of the main trunk of the chyle vessels still makes some scientists doubtful; if it could be demonstrated in the liver the problem would be solved, etc." And Riolan [6] says in his *Enchir. anat.* (Ch. 18): " Solely the diversity

in their distribution causes concern to many. For on opening the abdomen of a living animal recently fed these lacteal vessels are seen to be scattered through the mesentery, some going to the pancreas, some to the liver, while still others are diverted to the trunk of the vena cava, but, none at all to the spleen. They do not come together, like the portal branches, into a common trunk. They seem rather to have their root and origin in the pancreas and to be distributed in all directions from there."

I think that the structures shown by Vesling [9] in a picture of the dissected embryo and by Nathaniel Highmore [1] in his excellent illustrations, were really small nerves, for they did not support their opinion either by inserting a stilet, by inflation, ligature, or by movement of fluid.

The reason for the idea that some branches of the lacteal veins empty into the liver, though they have never with certainty been seen to do so, is, I think, an unwillingness to give up the ingrained opinion of physicians and philosophers that the liver is the blood-forming organ. This wide-spread and long-standing difficulty was dispelled in 1650, when I happened to be present at the slaughtering of a calf. The veins and arteries of the neck as well as the trachea and oesophagus had already been severed, and the organs were in the process of being removed. I closely examined the open thoracic cavity in order to get definite knowledge of the motion of the heart in the exsanguinated animal. While the women who were doing the slaughtering were roughly manipulating the intestines, a fluid like milk-whey ran out here and there in the region of the neck. Thereupon, after getting the eviscerated carcase from them, *I forcibly pushed the aorta to the right and discovered a duct filled with a milk-like fluid.** Dissecting upwards from the region of the diaphragm to the neck I separated the aorta from the intercostal branches and the vertebra. Now the duct was plainly to be seen—all except its insertion (thanks to the women having lacerated the vessels coming from the heart). I again focused my attention on the diaphragm, freeing its right side, and then came across a small vesicle filled with what looked like watery milk. The removal of the viscera prevented further investigation.

* Italics here are the translator's. This records Rudbeck's independent discovery of the thoracic duct.—Ed.

During the rest of the year I was busy with other work and could do no dissections.

The following year I opened the abdomen of a cat, five hours after a full meal. In order that the chyle should not run off quickly, I placed two ligatures on the lacteal vessels, one above, the other below the pancreas where the mesentery is attached to the back. Then I turned to the thorax, opened it, removing the sternum, and ligated the same duct which I had seen the previous year in the calf. After freeing the mesentery connecting the intestines with the lower lobe of the liver, I pushed aside the intestines toward the left, and opened the ligature below the pancreas. As a result the chyle flowed out of some small vessels into a vesicle situated between the diaphragm and the kidneys, posterior to the vena cava and the aorta, the vesicle becoming distended. I again tied this ligature and released the upper one; the result of this was that the upper chylous vessels emptied, while the lower ones became filled as far as the ligature. When, in turn, the ligature below the pancreas was loosened, the chyle immediately forced its way into the vesicle.

Then I opened the right chamber of the heart and tied off the axillary and jugular veins. Having carefully expressed the blood out of them with my fingers, I loosened the thread which obstructed the chyliferous duct below the heart, whereupon the chyle poured with great rapidity into the axillary vein at its junction with the jugular. From there it ran down the vena cava, being prevented by the valves of the vein from flowing upward, and gave a whitish color to the right chamber of the heart.

In April 1652 I demonstrated by dissection this chyle vesicle, not only by command but in the presence of Her Majesty, at which time the professor of medicine Stenius gave an address. On this occasion the royal physicians told me that not very long previously Pecquet [7] had come to Stockholm while preparing for the press his own study on the same subject, a copy of which I saw in June. Shortly thereafter the Jansonius firm brought out Van Horne's "Novus ductus chyliferous," [8] published in May. Finally, during the September fair in Stockholm I obtained the new work of Thomas Bartholin [9] on his discovery of the same vesicle. I say no more about the publications of the many authors, except that their experiments and observa-

tions make it quite clear that the liver is not the primary blood-forming organ.

But just as in ordinary cooking, not only fire but also pots, spoons, spatulas, etc., are necessary in order to clarify and skim off the soup, so the heart with its vessels, even though most important, does not in itself suffice to make the blood fit to nourish the body, but needs help from other organs like the spleen for fermentation, the kidneys for excretion of water, the liver for bile, and needs, doubtless, the separation of this new fluid.

I thus consider the main tasks of the liver to be as follows:

1) To skim (as in cookery), the bile from the blood reaching the liver after being fermented in its passage through the spleen, and also to carry the bile to the intestines through the bile ducts. The gallbladder, the branching of the bile ducts throughout, and the movement of the bile are evident proof of this.

2) To separate our "aqueous" fluid from the blood, as it were by filtration, and to deliver it to the chyle vesicle by way of our "hepatic" ducts. Anatomy makes this clear.

That these are the functions of the liver is confirmed not only by experience but also by the publications of many anatomists and physicians, especially by what Johann Vesling [6] says in ch. 4 of his *Syntagma anatomicum*, p. 40: "Although there are animals whose liver is red as long as there is blood in their veins, in reality it is white, yellow or green." He thus intimates that the liver is red in those animals only whose blood is not mixed with strongly yellow or black bile to such a degree as to color the liver substance by its secretion; that bile color can be masked by an excess of blood may easily be shown experimentally. But in other animals, if the blood is flooded with yellow bile, the liver is also yellow; and similarly blood replete with whitish aqueous humor makes the liver white, provided the respective humors are excessive enough to saturate and tinge the whole parenchyma.

André Du Laurens [2] seems to agree with this in his anatomical work, ch. 17, question 22, p. 156: "If the redness of the liver comes from its substance, why is it that its products are not red, but either yellow or black?" In other words, if the liver were the blood-forming organ, the blood would doubtless assume the same color as

the liver itself; whereas the liver has not always the color of blood, but often that of the humor which it purges out from the blood.

And Riolan [5] in his *Enchiridium anatomicum*, ch. 24, fol. 125, says " the liver is reddish and gives that color to the blood, although in some animals the liver is of a different color, namely green, black, or yellow; in them the blood gets its red color in flowing through the substance of the heart." In the last clause he seems to refute his own opinion about the coloring of the blood by the liver.

Experience also bears this out. In several places in his book on the generation of animals Harvey [10] states that the blood appears in the heart before any vessels are formed, and that it is not until long after this formation that the liver attaches itself to the umbilical vessels by a sort of exudation and condensation. He observed this in both oviparous and viviparous animals. This makes it evident that the redness of the blood is not dependent on the color of the liver, since the blood is formed much earlier than is the liver.

So I think that this work of nature, the formation of the blood, takes place as follows:

In the first place that blood which is not used up in nutrition is carried, along with the chyle, to the heart by whose heat and motion it is matured, colored, attenuated and distributed. This is shown by the fact that in the first stage of development blood is to be found only in the heart, which, consequently, must be acknowledged to be also the primary chyle-forming organ. Then before the blood is distributed to the arteries, it is charged with various humors which must first be digested by the heart or they will reach the organs in an immature state and do them harm. Some blood flows upward to the head and is freed of phlegm, according to Hippocrates and others who have called the brain a large gland; and some goes downward to the kidneys and, after discharging fluid to the urinary bladder, passes on through the renal veins to the vena cava. Thence part of it enters the spleen to be fermented, as de Wale [11] seems to have clearly proved in the first of his " Epistolae " to Bartholin, pp. 445, 446, ed. 2.

From there the fermented blood is carried by the splenic vein to the liver, the parenchyma of which absorbs it and, by a sort of filtration, separates from it the residuum of the splenic fermentation and the various biliary juices, as well as the aqueous humor. These

fluids are collected by the liver in the gallbladder and the biliary and hepatic ducts, through which it discharges them. It is not strange, therefore, that the liver is red in most animals because the blood greatly exceeds the humors in quantity and should give its color to the parenchyma; but in those animals whose blood is very rich in either biliary or aqueous humor, the liver is stained with a corresponding color.

Fermented in the spleen and with its waste products eliminated in the liver and other viscera, the blood finally becomes suitable for nourishing the body, returns from the liver to the heart, and is distributed throughout the body by the arteries. The body converts into its own substance what it can use, and sends the rest back through the veins to the heart and viscera for further elaboration.

It is difficult to determine the nature of this aqueous humor which is poured by way of these hepatic ducts into the chyle. I leave the learned to decide whether it acquires, like the blood, some fermenting faculty in the spleen and for that reason is carried to the chyle, after passing through the splenic vein to the liver which excretes it. I am equally uncertain whether it resembles the bile in conveying movement to the chyle, and in promoting the evacuation of solid excrement; or whether its aqueous consistence is merely for the purpose of diluting the chyle, which might otherwise become too viscous. Without prolonged investigation it would be extremely difficult to make any definite and positive statement about the function of this fluid.

Chapter IV

The medical significance of this discovery.

It is generally acknowledged that the liver may be attacked by various kinds of diseases; amongst these however physicians have never been able to agree as to the nature of ascites. I wish, therefore, to draw particular attention to the observations and opinions of those anatomists who assign to the liver a role in the origin of this disease; a different cause I shall discuss at the end of this treatise.

Hippocrates, in his 55th aphorism, book 7, thus describes the nature and cause of this disease: " In cases where the liver is filled with water and bursts into the omentum, the belly fills with water

and the patient dies." On which Galen [13] comments as follows: " The liver is very apt to produce, in the membrane covering it, the little watery vesicles commonly called hydatids. The liver of slaughtered animals is sometimes found completely studded with them. Should these happen to burst, their water is poured into the peritoneum in the epigastric * region." Fernel,[15] in his *Universa Medicina,* bk. 6, ch. 8, agrees that vesicles are formed under the membrane surrounding the liver. Sennert [14] (bk. 3, part 6, sect. 1, ch. 7) states that in animals as well as in man he has observed these water-filled, transparent vesicles, varying in size and number. In dissecting a young woman Plater [15] found attached to the liver, a large fungous tumor from which a serous humor exuded.

Adrian Spigelius [16] says in ch. 12, bk. 8, " The liver is joined with the mesentery, the portal vein, the ribs and the vena cava, but with the omentum by that small narrow lobe through which, as we have previously seen, water pours out from the liver into it "; further on, " The parenchyma of the liver is protected by a very thin and simple membrane, derived from the ligaments of the liver and intimately connected with it. If at any place this membrane loosens, a watery material escapes from the vessels and collects between the membrane and the liver substance, thus causing what the Greeks called hydatids. When the latter burst, their contents escape into the abdominal cavity and give rise to ascites."

Thomas Bartholin [17] says in his *Anatomia reformata,* bk. 1, ch. 14, " in this liver membrane water vesicles appear, producing ascites according to Plater. In a goat I have found a great number of such whitish vesicles, which, when opened, were found to contain a peculiar serum, mixed with mucus, and also a different yellowish substance, due either to congenital defect or to domestication."

Evidently the cause of the vesicles has been diligently investigated, the various explanations being due to ignorance of these ducts of the liver. For when these are obstructed, the membrane covering the liver becomes elevated in several places by fluids which would otherwise flow to the chyle vesicle, as I had occasion to observe during the dissection of a cat on 22 February of this year. In this animal nearly all the liver ducts, and particularly the lower ones under the portal

* Thus Galen; Rudbeck has " hypogastrium."

vein, became filled with a very thick, viscid juice, and the liver itself became swollen with vesicles full of whitish-yellow fluid. When these vesicles were opened, fluid of the same color escaped.

Thus some physicians agree that vesicles are produced in the liver and that their rupture gives rise to ascites. But how and why they are produced, many authors offer no suggestion, while those who have ventured to give some plausible explanation, have expressed such divergent opinions that they leave the reader equally in doubt. My own experience has taught me the explanation best fitting the facts, and I would assert that the vesicles are produced by obstruction of the hepatic ducts and a consequent backward flow of their contents to the liver.

It is clear that the liver is subject to other disorders as a result of changes either in the liver itself, or in its blood, or in these hepatic ducts.

First. It appears that a slight obstruction of these vessels and an excess of fluid may produce an abnormal condition in the liver.

Second. The size of the liver may be changed, if the humor pervading the parenchyma becomes inspissated as a result of the obstruction.

Third. Its outlets are involved in any blocking of these channels, if the latter may be considered belonging organically to the liver.

Fourth. Even with these channels obstructed it is probable that the liver can still distribute their fluid throughout the body by returning it, along with the blood, through the veins and arteries.

Fifth. Various abscesses are produced in the liver by the putrefaction of this fluid.

Sixth. Its deterioration and overabundance give rise to hepatic diarrhea.

Seventh. If corrupted in the liver, obstructed in its own channels and circulated with the blood, it causes a general bodily cachexia.

I shall not weary the reader with other conditions that may depend on the altered relation of the liver to this fluid and its ducts.

Chapter V

Anatomical methods.

To remove any remaining doubt and to facilitate the work for the novice, I propose to describe my method of investigating these ducts.

For this purpose it is best to use the dog, cat or fox, since in the sheep or calf the great bulk of the intestines makes the search more difficult. After opening the abdomen, remove the omentum or reflect it over the thorax; then place a ligature between the stomach and the liver in that portion of the mesentery which connects those organs and the intestines, which in nearly all animals is entirely free and which contains within it the portal vein and the bile duct. Between the liver and the ligature one immediately sees ducts filled with aqueous humor, which in an hour or so turns yellowish. Freeing the intestines from the lower lobe of the liver adjacent to the right kidney, and pushing them aside to the left, separate the colon from the lumbar spines and tie the chylous vessels which run alongside the portal vein and immediately below the pancreas glandulosum in that part of the mesentery connecting the intestines with the back. Then turn to the thorax, remove the sternum, and tie a thread round the thoracic duct, after first emptying the chyle duct, and similarly in the abdomen shut off the distal end of this duct in order to prevent the serous vessels, which have a tendency to run in that direction, from filling the vesicle. Sometimes, however, the latter ligature is unnecessary, because the serous fluid often remains in its vessels. Place one more ligature in the mesentery near the chyle vesicle, loosen the first ligature between liver and stomach and one sees, as illustrated in Tab. I., *m*, how the gland receives these ducts, as well as how they become filled with fluid down to the lower ligature. If this ligature is now loosened, then the whole chyle vesicle becomes filled with the same fluid; and if, finally, the ligature in the thorax, constricting the thoracic duct a little above the vesicle, is also loosened, the chyle vesicle empties and, if the veins of the neck are first tied off and an opening made in the right ventricle, one can see the fluid entering the axillary vein and heart.

To see the vesicle fill with chyle, tighten the thread placed in the chest around the thoracic duct and loosen the one around the chylous

vessels running toward the pancreas. By alternate opening and closing of these ligatures one can thus at will fill the chyle vesicle with the aqueous humor or with chyle.

It is not my intention, nor have I the time to go further into the nature of these liver ducts. Therefore I shall proceed to an equally brief explanation of the remainder of my subject, namely the serous vessels of the glands.

Chapter VI

The dates of the discovery of the serous vessels of the glands.

On 27 January 1651, while investigating the hemorrhoidal veins of a dog, I noticed some vessels filled with serum situated under the colon and rectum, and tracing these ducts upwards, I found that the serous fluid they contained flowed into the chyle vesicle.

Then on 8 February of the same year, when I was dissecting away the oesophagus from the aorta and the dorsal vertebrae in a calf, and also in a sheep, there came into view a large gland attached to the oesophagus near the diaphragm and connected with a long vessel full of serum. At first I was uncertain whether this might not be the chyle duct; but when I removed the aorta, that duct came to light and resolved my doubt.

On 6 March 1652, when examining the anastomoses between the spermatic veins and arteries in a sheep, I observed, twice in the same day, numerous vessels, transparent, nodular, swollen with serum, covering the surface of some glands alongside the crural veins, and running towards the chyle vesicle.

On 19 October, while demonstrating the abdominal arteries and veins of a cat, I saw other serous ducts accompanying the lumbar veins beneath the kidneys, and with their branches extending over a large part of the side of the abdomen and scattered here and there between its transverse and oblique muscles.

On the 30th of the same month in the opened thorax of a dog with the lungs and heart pushed aside to the right, I noticed two glands in the mediastinum between the lungs and the heart, sending a duct under the heart direct to the chyle duct.

Finally and by chance, on 27 April 1653, when I was demon-

strating to some friends the opening of the thoracic duct into the axillary vein, small glands appeared posterior to the sternum near the clavicle between the mammary veins and these glands sent two small branches filled with serum to the thoracic lacteals.

Chapter VII

The name, character, origin, etc., of these vessels.

It is my intention to call these ducts the "*vasa glandularum serosa.*" In the first place "vessels," because in shape, appearance and purpose they conform to other types of vessels; secondly "of the glands," because many of them take their origin from such; and thirdly "serous," because of the fluid they contain.

Concerning their nature, temperament, quantity, appearance and relations with other parts of the body nothing in particular needs to be said. Membranous, containing numerous valves, and externally nodular, etc., in all respects they resemble the hepatic ducts.

It is fitting to consider their origin and site together, since these canals so to speak insinuate themselves into various parts of the body. Those found in the neck region spread their branches behind the sternum, between the fat (Tab. II., fig. 1, *G* & *O*) and the veins in the mediastinum; there one also may find one or more glands (Tab. II., fig. 1, *O*). At the upper part of the sternum these branches are joined together forming one or, sometimes, two tubules (Tab. II., fig. 1, *pp*), situated anterior to the axillary vein and entering into the thoracic duct near its upper end. At times one sees here a similar tubule accompanying the right axillary vein and emerging from the right ribs (Tab. II., fig. 1, *q*).

The serous ducts on the left side of the heart send branches to the pericardium (Tab. II., fig. 1, *r*), its fatty tissue and the mediastinum (Tab. II., fig. 1, *A, B, r*); they then unite in some small glands, cross the aorta (Tab. II., fig. 1, *s*), and enter by a small branch into the thoracic duct.

Others, emerging with a profusion of branches from between the transverse and oblique muscles of the abdomen (Tab. II., fig. 1, *hhhh*) and apparently unconnected for the most part with any gland, join one another at a distance of about two fingers' breadth from the

vena cava, and accompany the lumbar veins to end in the chyle vesicle. As far as I can see, they lie between the layers of the peritoneum.

Those serous tubules which accompany the crural veins (Tab. II., fig. 1, *gg*) and empty into the chyle vesicle posterior to the vena cava pass through and around two or three glands located in the groin (Tab. II., fig. 1, *fff*). They are larger than the other serous ducts in all animals I have examined.

Finally, the serous vessels behind the colon (Tab. II., fig. 1, *eeee*) seem to derive from some small branches in the fatty tissue between the sacrum and rectum. These branches unite a little below the bifurcation of the vena cava and run along the surface of the iliac veins (crurales) to end, like the other serous vessels, in the chyle vesicle.

I have not illustrated the serous vessel which is to be found behind the oesophagus in calves and sheep because there was scarcely room to include it on the plate.* Some description of it, however, should not be omitted. It takes its origin in many roots from a large oblong gland, which nature has placed alongside the oesophagus near the diaphragm; from there it ascends on the right side of the oesophagus and joins the thoracic duct.

They vary in number, sometimes numerous, sometimes few; sometimes smaller, sometimes larger. Their color is watery, that of the liquid visible through their transparent walls.

It is to be noted that these serous vessels vary not only in different animals, but also in different individuals of the same species; so we see nature playing tricks with these vessels as well as with the veins, arteries, nerves and lacteals.

Chapter VIII

These vessels and the function of their fluid.

It is generally acknowledged that nature has assigned her own particular tasks to all the organs of the body and that she does not unnecessarily duplicate them. And I believe, accordingly, that these vessels have been developed to perform some function. For let us

* An illustration of this vessel is given in a later edition of Rudbeck's thesis.

consider their appearance, shape and structure; they are hollow and tubular, and have innumerable valves, all opening towards the chyle vesicle or the thoracic duct, so as to prevent any backward flow of the fluid drawn from the glands or elsewhere, as can be plainly demonstrated by ligature. Consequently, their use or function will be to convey a fluid from the glands and other organs to the lacteal reservoirs and ducts. Because when ligated, they swell up on the glandular side towards their origin, but collapse on the side towards the lacteal ducts.

In view of the close relationship of most of these vessels to the glands, which they surround and penetrate with their branches, I think it would be well to bring up the question, briefly, of the function of the glands.

I. I wonder whether they were not designed to attract the serum exuded by the heat of the intestines and other internal viscera, and to transfer it to the chyle vesicle so that the kidneys may separate it from the blood along with the serous constituent of the chyle.

II. Or perhaps nature intended them to draw out the chyle from the blood and gave them the capacity of the pancreas glandulosum to prepare, digest and filter it anew, and then to deliver it to the chyle vesicle by way of our vessels. Thus it would circulate like the blood until it acquires the nature of blood and serves as a nourishment for the body. But here you may ask why these vessels, if they contain chyle, are not whitish like the lacteals? The answer is that the chyle does not always have the same character and color, particularly when it has circulated in the blood vessels through the viscera and has thus become greatly diluted. And so our vessels tend to have a watery color due to the aqueous fluid visible through their extremely thin walls; indeed, even the lacteal veins of the mesentery and thorax frequently present a similar appearance.

III. The glands are embedded in fat which is liquefied by the heat of the body and constitutes, according to physicians, the oily part of the blood. One of their uses may be to take up the fat or, if you prefer, only the serous humor from it, and to convey it to the chyle vesicle for further concoction so that it may be made suitable for nutrition. I am inclined to think that this idea deserves closer study. In fevers and chronic diseases these glands may well convey the

liquefied fat through the serous vessels to the chyle vesicle to serve as nutriment to the body when the patient, owing to his various symptoms and the weakness of his digestive organs, eats scarcely anything.

However, it is very difficult to say in what diseases a mixture of this fluid with the chyle would be useful to the latter. For if it be granted that the fluid is serous and produced by imperceptible exudation, I would not positively deny that it may be carried to the chyle vesicle for the sole purpose of diluting the chyle and facilitating its excretion from the body. But if this juice is circulated chyle, or liquefied fat, it means, I think, that it is given the benefit of circulation with the constant accretion of chyle, for the purpose of its further elaboration.

To be perfectly frank, I will not venture to express anything more definite than probabilities about the function of this fluid until further experience shall have blown away the mists from my eyes and those of others.

Chapter IX

The significance of this discovery for medical practice.

I think it not unlikely that pathological changes in these vessels may give rise to disease and particularly to ascites if they become obstructed by viscid, thick or mucoid fluid. Their occlusion causes swelling of the glands and other sources of the serous fluid which is forced backward instead of flowing from them through these vessels to the chyle vesicle. The swelling increases daily as long as the vessels can stand the pressure. Finally they burst and discharge the serous fluid into the abdomen or the chest. I happened to observe this condition in a sheep on 9 April of this year. The glands about the iliac veins in the abdomen were rather larger than a closed fist, soft and swollen with water externally, but internally quite firm (Tab. II., fig. 1, *fffff*). Their serous vessels near the bifurcation of the vena cava and aorta into the iliacs, were compressed by a firm tumor between which and the glands they were as thick as a quill, while there was no trace of them above, towards the chyle vesicle. In the abdomen I found six pounds of a watery fluid on which the intestines

floated. When I cut the serous ducts the fluid immediately escaped from them and from the glands and when I squeezed the remaining serum out of the glands, they became smaller than a bean. From this I inferred that the water had been extruded into the abdomen as a result of the obstruction to the vessels preventing its removal through them from the glands to the vesicle. I think, therefore, that any glands which have these vessels may cause a watery dropsy. Rondelet [18] too, seems to be of this opinion (ch. 35); " In frequent dissections I have observed that the glands of the mesentery, likewise the pancreas, are indurated in all dropsical patients."

Similarly any obstruction of the serous vessels running between the abdominal muscles (Tab. II., fig. 1, *hhhh*) and peritoneum, might give rise to local vesicles whose rupture would cause a dropsical swelling between the skin, muscles and peritoneum. Nor does it seem unlikely that water might gather between the sternum and the mediastinum through obstruction of the vessels which come from the sternal glands in the interclavicular region and empty into the thoracic duct near its entrance into the axillary vein (Tab. II., fig. 1, *o, p*); and the same thing might happen in the parenchyma of the lung and in the thoracic cavity through the occlusion of those running to the thoracic duct from the pulmonary glands near the left side of the heart and in the pericardial fat (Tab. II., fig. 1, *s, r, s*).

It should further be noted that the accumulation of water in the pericardium interferes with the action of the heart and may finally choke it, which may well be the result of stoppage in our vessels (Tab. II., fig. 1, *r, sss*) if their function is to drain serum from the pericardium into the thoracic lacteals. Let us quote here one or two cases from Sennert [19] who grants that small watery vesicles may be produced almost anywhere in the body: " I cannot refrain," he says (bk. 3, part 6, sect. 2, ch. 3, fol. 709), " from pointing out that sometimes the water is not in the peritoneal cavity but in vesicles attached to the abdominal viscera. Such a case is reported by Maurice de la Corde (Comm. 5 in bk. 2 *de mulier.*); that of a dropsical woman who died in Paris on 8 March 1567. At autopsy every single space in the body was entirely obliterated by a vesicle conforming in size and shape with the cavity it filled. Inside and outside of the hollow organs, not only the bladder, but the kidneys, uterus, even the

stomach, the intestines, the heart, pericardium and the rest, pendulous cysts were everywhere to be seen filled with a yellowish fluid which remained odorless even after 20 days. Neither was the liver or spleen free from this, nor the organs above them up to the neck region. They were estimated to exceed 800 in number. In structure they were composed of two membranes, an inner whitish one, and the other resembling the outer coat of the stomach, like it in color but somewhat thinner."

Schenck (Obser., bk. 3) reports a similar case communicated to him by I. Eichholtz. " In the year 1581 a poor woman about 40 years of age died, as it was thought, of dropsy. When the body was opened, neither water nor air was found in the abdominal cavity, but the intestines and other internal organs were compressed within a narrow space. The abdominal wall, from skin to peritoneum, presented the appearance of white fat without any intervening muscle tissue, but was composed of numerous vesicles, large and small, matted together and filled with a limpid fluid. Elsewhere there was not much variation from the normal, though the omentum was very small and scarcely covered the intestines. The liver, whitish externally, was of normal size and consistency."

Thus, in various pathological conditions of these serous vessels aqueous fluid may be poured into the chest, abdomen and viscera, or vesicles may be produced, by displacement, compression or obstruction of the vessels by a thick fluid, a stone, worms or pus, with consequent back flow, rupture or oozing through their walls.

Chapter X

How to find these serous vessels.

Of the many structures difficult to find in anatomical dissections, these vessels, I must confess, are by no means the least. For usually they will not tolerate the finest blunt probe, a sharp knife, a suction tube, or any other instrument whatever. And even though abundantly present, they are often obscured by fat, or are overlooked if not at the moment filled with fluid. When seen they may disappear if not quickly ligated. Thus in elusiveness they rival the lacteals and must be handled with utmost care.

In the cat, dog, fox, sheep, goat etc., one can demonstrate them during life as well as after death, but must be careful not to let the hemorrhage hide them in the living subject or the fluid escape quickly in the dead one. Moreover, avoid using a very fat animal or the search may be vain, though sometimes successful.

On opening the abdomen of the animal, if you wish to inspect the vessels arising from the glands about the iliac veins, separate the mesentery carefully from the lumbar region, put a ligature round it, including the vena cava and aorta at their bifurcation near the sacrum, and immediately you will see the serous vessels swell up, provided the time is opportune and there has been no laceration of these or of the mesenteric vessels.

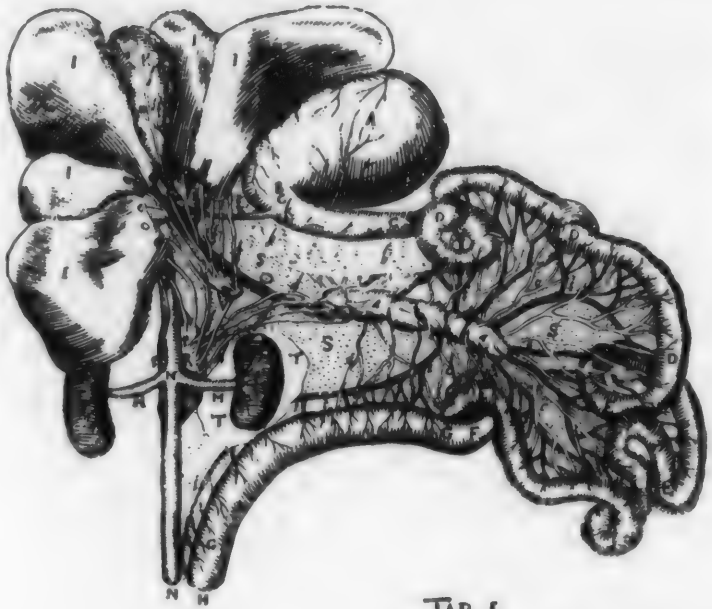
The other vessels, running between the colon, rectum and sacrum, can be seen with the same ligature.

But to see those which accompany the lumbar veins and whose branches are concealed by the abdominal muscles, carefully dissect away from the lumbar muscles only that part of the mesentery which lies nearest to the vena cava, and put the ligature around it and the lumbar veins and arteries. If this is properly done, the small branches gorged with fluid will come into view between the ligature and the muscles.

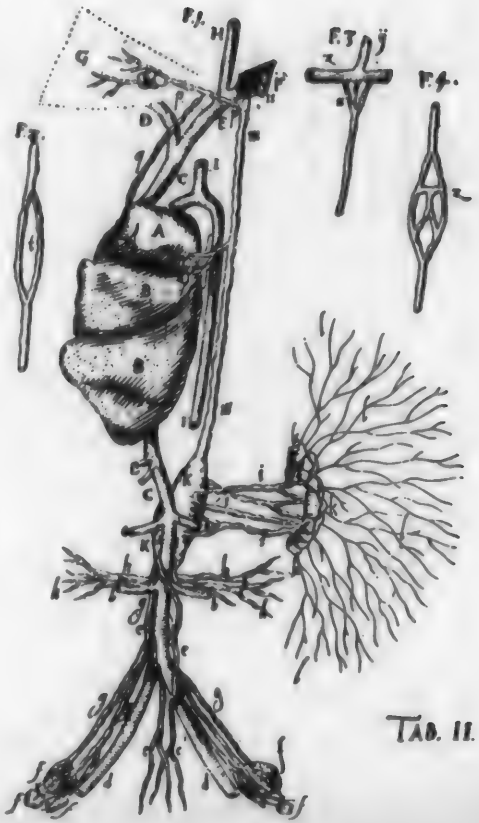
If it is desired to inspect the serous vessels in the thorax, detach the sternum from the clavicles and turn it back; then place a ligature near the opening of the thoracic duct in the neck region around the mass of mediastinal fat supported by the thymus; whereupon they will become evident, back to their origin in the glands.

With this same procedure, by simply pushing the heart and lungs to the right, you can also see the vessels which originate from the glands of the lungs and the pericardium. Here, however, I cannot indicate any one spot where the ligature ought to be placed if they do not quickly appear. Often they may be discerned without any ligature, as is not infrequently the case in a search for the other serous vessels.

All this could be amplified in many ways and embellished in more elegant language; but I hold that the simplest presentation is best suited to the truth. Nothing, however, would please me more than to have some one of richer experience find out more about the function of these structures, to the advantage of the anatomical world.



TAB. I.
sculpsit Magn. Celsius



TAB. II.

Explanation of plate 1

A, dog's stomach; *B*, pylorus; *C*, duodenum; *D, D, D*, jejunum; *E, E, E*, ileum; *F*, appendix; *G*, large intestine; *H*, beginning of rectum; *I, I, I, I, I*, the liver's five lobes; *K*, gall bladder; *L, L*, the kidneys; *M, M*, the veins of the kidney; *N, N*, vena cava; *O*, vena porta; *R*, the chylous receptacle; *S, S*, mesentery; *T, T*, the mesentery partly cut through, in order to facilitate the ligation of the efferent ducts of the liver.

a, a, pancreas glandulosum, in the center of the mesentery; *b, b*, pancreas carnosum, attached to the duodenum and situated below the stomach; *ccccc*, chylous vessels between the intestines and pancreas glandulosum; *ddd*, chylous vessels, which emerge from pancreas glandulosum and enter into the chylous receptacle; *aa eeeee*, the beginning of the liver ducts outside the liver; *fff*, these ducts in their course to the gland *m* and their continuation from there to the chylous receptacle; *gg*, two branches of the bile duct; *h*, this duct's entry into the duodenum; *iiiii*, veins of the mesentery; *m*, a gland, situated below the vena porta, which receives the efferent ducts of the liver; *nn*, an often occurring duct which creeps along the gall bladder; *oooo*, vena porta's ramifications in the liver; *rrr*, liver ducts, which are firmly attached to the vena and enter into the chylous receptacle; *tt*, veins of the gall bladder.

Explanation of plate 2

Fig. 1. *A*, the heart from the left; *B, B*, the lungs turned over to the right; *CC*, the vena cava; *D*, the right axillary vein; *E*, the left axillary vein; *F*, one portion of this opened, in order to show the entrance of the thoracic duct; *G*, the sternum, designated only by points; *H*, the left jugular vein; *JJ*, aorta; *KK*, the chylous receptacle; *L*, branches of the vein of the liver.

aa, the veins of the kidney; *bb*, the lumbar veins; *dd*, the crural veins; *eeee*, serous vessels, which posterior to the rectum run to the chylous receptacle; *fffff*, glands situated next to the crural veins; *ggg*, serous vessels, which go from these glands to the chylous receptacle; *hhh*, vessels of similar nature, which run between the abdominal muscles and likewise enter into the chylous receptacle; *iii*, chylous vessels, creeping between pancreas glandulosum and the chylous receptacle; *kkk*, pancreas glandulosum; *lll*, chylous vessels in the omentum between pancreas and the intestines; *mm*, the thoracic duct; *n*, its entrance into the axillary vein; *o*, glands of the sternum; *pp*, serous vessels of these glands entering into the thoracic duct; *q*, a branch of the last mentioned vessel, which goes to the ribs; *rr*, glands of the heart; *ss*, serous vessels of these glands, which posterior to the heart enter the thoracic duct.

Fig. 2. *t* shows the thoracic duct's two divisions posterior to the heart as I have often observed it.

Fig. 3. *x*, the axillary vein and the left cervical vein; *y, u*, triple entrance into the thoracic duct.

Fig. 4. *z*, abundant ramification of the thoracic duct, as has been observed three times in animals.

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NOTES AND COMMENTS

ROBINSON'S[1] "THE FOUNDATIONS OF PROFESSIONAL DENTISTRY"[2]

BERNHARD WOLF WEINBERGER

The task before me is neither a pleasant nor an easy one to perform, for if the remarks made by the author in his Preface are in accord with dental history, the project would be a worthy one and should receive the acclaim of all those interested in the subject. The author's desire was " to present logically and accurately the series of important events and movements in history that have brought dentistry to its present state." He feels that in doing this he " has made some valuable contributions to the facts of dental history and that his modest efforts may serve to stimulate further research that may ultimately produce a comprehensive and authentic history of dentistry."

How valuable and authentic a contribution it is, only those thoroughly acquainted with the subject are in a position to evaluate its true worth. The profession accepts it on confidence alone, and in " reviews " praises it because of the author's position and standing in the profession. It now has become the foundation for further articles and study, with all of its errors, misstatements and unwarranted deductions to be repeated. For that reason I feel it is my professional duty to bring to the profession's attention some of the mistakes, and show that it was not " written in the light of the best available knowledge " as claimed on page 975.

Having had published since 1924 some twenty-five papers in both medical and dental journals that related to the subjects under consideration in the " Foundations," I believe I am in a position to judge what is history and what is fiction.

[1] Dean and Professor of Dental History, the Baltimore College of Dental Surgery, Dental School, University of Maryland; Chairman, Committee on History of the Dental Centenary Celebration; Member, Committee on History of the American College of Dentists; Member of the Maryland Historical Society.

[2] Sec. X. *Proceedings Dental Centenary Celebration*, Baltimore, Maryland, March 18, 19, 20, 1940, pp. 973-1048.

In 1933, under the title of "The Cause of Errors in Dental History" (*Dental Cosmos*, pp. 333-347) I had occasion to reply to another article by the same author. The same issue contained an editorial and on page 397 the following appeared:

> When one becomes sufficiently interested in the history of dentistry that he feels impelled to write on the subject, he assumes considerable responsibility to those who follow in endeavoring to attain meticulous accuracy of his data, quotations and bibliographical references used as the source of his material. One of the most annoying incidents in the life of the historical searcher, or any other researcher for that matter, is, after considerable trouble and effort in locating a reference, to find that it is incorrect and useless. Such an outcome shakes the confidence of the reader in the author of the article and is certainly not encouraging to the utilization of that author's work. To this one fault may be attributed much of the difficulties of the present-day historian, as pointed out so specifically by Dr. Weinberger.

One might ask whether the author should have set himself a task of such scope and responsibility when advised by those he consulted in his letter of December 2, 1938, that the time was not sufficient to prepare an accurate and worthwhile history and that they therefore could not cooperate. Likewise whether it was proper for the author, under the circumstances, to utilize this section of the "Proceedings" for the exclusive advocacy of his own historical "pet" theories. Likewise whether the author has the privilege, in writing history, to ignore, misinterpret and disparage the writings of his fellow associates in order to write a "comprehensive and authentic history of dentistry." One of the requirements of an historian, according to Sigerist,[3] is that he must "submit to the iron discipline imposed upon him by the historical research methods." In this incident I feel very much like Trueman,[4] who upon a similar occasion wrote: "It is much to be regretted that the dental historian is hampered at every turn by carelessly written articles such as this in review. A little research by their writers would show whether their statements were facts or fictions. Anyone unwilling or unable to make such needful effort has no business to write upon history at all."

[3] Sigerist, Henry, *Bulletin of the History of Medicine*, 1939, p. 357.

[4] Trueman, William H., "A Few Errors in Dental History Corrected," *Dental Review*, 1901, p. 129.

An historian who writes for an "occasion" such as a centenary celebration, assumes a special obligation. Here he finds that he has to meet the particular needs of special interested groups, and his position becomes not unlike that of a physician. Since the reader, as a rule, cannot critically evaluate the historian's work but must accept it on confidence alone, the historian's responsibility toward him therefore is no less than that of a physician towards his patient. From a physician one expects in his relationship with his colleagues, professional etiquette; in his relationship with his patients, professional ethics. Is the parallel forced?

How are the readers of the "Foundations" injured, and the history of dentistry altered, by careless copying of names, dates, facts and important omissions of known items? Unwarranted deductions and conclusions are the result. If the author misquotes a statement made by another, how then is the reader affected? Does "disorders or diseases of the teeth" convey the same idea to the reader as "disorders or deficiencies of the teeth"? By changing the word "deficiencies" to "diseases" the author, on page 867, creates the idea that Fendall recognized that many diseases had their origin in the foci of infection. Why then is it quoted correctly on page 990? In what way is the reader affected when incorrect bibliographical references are cited, confusing the reader, as on pages 1014-1015; or when, in treating of the early dental literature and conditions in different nations, items are ignored. Certainly the reader cannot help but obtain an inaccurate story of dentistry's evolutionary development.

Besides the lack of sufficient time, many reasons might be given as to why the author has failed in this effort. First, his dependence upon others to do the actual research. Consequently he has turned over to him only that material which others consider important, so that much is overlooked that might later prove of inestimable value. At the same time he is unable to judge how correct and reliable this data is. This is undoubtedly the reason why the author failed to recognize in Fendall's advertisements the writings of John Baker and Thomas Berdmore, and why he quoted instead of from the newspapers of that date, John Greenwood's (p. 993) and Le Mayeur's (p. 997) advertisements found in an unrecognized facsimile 1876 copy of New York City Directory as that of 1786. There are no such

advertisements in the original edition. This likewise is the reason for his failing to recognize the misdated James Mill's announcement in the *New York Weekly Journal*, January 6, 1734. The preceding number bears the date December 29, 1734, and the one that follows, January 13, 1735. Therefore all historians are correct in giving it as of January 6, 1735. Second, the author being a part of the Harris-Hayden feud that he himself relates on page 1024, is unable to be freed of its influence so as to write an unbiased history. This is clearly shown in his attempt to dismiss, in a footnote on pages 1011-1012, the value of John Harris' contributions to dentistry. Finally the repeated ignoring or a lack of knowledge of the more recent and important contributions to dental history, has prevented him from making the work complete, authentic and from being " written in the light of the best available knowledge."

To each of the six chapters there is to be found a bibliography, and in all there are one hundred and two references. An analysis of these will bear out the above statement. In Chapter 1 " The Background " up to 1800, the author has listed eighteen items; six are from Guerini (1909) and one from Koch (1910), the only two dental histories utilized. Therefore we are to assume that all of the others are apparently worthless. Had he consulted some of these, however, he would have been more accurate and could have found more convincing and truthful evidence than he now submits. Garrison's *History of Medicine* supplied three, the writings of Hippocrates two, McCauley on Egypt and Monroe on education one each, leaving but four unaccounted, being dental books written between 1728 and 1848. Can the author's intention be to convey the idea that dental writers gave this important period little or no consideration. If that is so, one need only to consult the Dental Index Periodical (Black's) to find out otherwise.

How complete is the first chapter? While consideration is given to the Papyrus of Ebers, one wonders why the author stops there and ignores the Edwin Smith Surgical Papyrus, which is not only the oldest known medical writing but also the most complete and important treatise on surgery of all antiquity. The author informs his readers that " according to Guerini the earliest practice of the prosthetic art, of which there is acceptable proof, was among the

Ancient Phoenicians," because of discoveries of prosthetic appliances . . . p. 976. Yet he disregards the older, more valuable and more numerous examples of Etruscan bridgework that are to be found today in various museums in Europe. The reader is not informed that Guerini himself, on pages 67 and 68, wrote: "Nevertheless, in comparing the dental appliances found in the Etruscan tombs with the *sole authentic* dental appliance of Phoenician workmanship known at the present day, we cannot but be struck with the great superiority of the Etruscan appliances." Since Guerini there have appeared in our literature many important contributions relating to these prized specimens, of which the author is apparently not aware.

How accurate is the author when, on page 976, he remarks: "There is no convincing evidence of a dental art among Babylonians, Assyrians . . . or Hebrews"? L. Parmly Brown, in the *Dental Cosmos*, 1934, in his series of articles on "Antiquites of Dental Prosthesis," ignored by the author, and which might have been used on many occasions, shows otherwise. Certainly B. R. Townend's "An Assyrian Dental Diagnosis" published in the *Iraq*, 1938, and mentioned by him in the *British Dental Journal*, 1937, supplies the evidence as to Assyrian dentistry, which certainly is convincing. Here we have the earliest reference to the problem of focal infection and the removal of teeth to cure disturbances in the joints, etc. Though we read on page 981 that "the first dental text book of value was published in Germany about 1530" with an English title "Medicine for the Teeth, etc.," why is there no consideration given to its importance and its relation with the medicine of that period, especially since we have fourteen known editions between 1530 (a date that is definite) and 1576, and all with German titles. Can a history of dentistry be considered complete when the first dental book to be written in English is not worthy of mention? This work by Charles Allen went through three editions, 1685, 1686, and 1687, and has been mentioned by many since 1924. Many more examples might be given to illustrate that in this first chapter the data used was entirely from writings prior to 1910, that an authentic history has not been compiled, that it lacks completeness and that material has been ignored.

The other chapters could be reviewed in a like manner but lack

of space will not permit this. A study of the 102 references given shows that excluding Guerini's seven and Koch's five, with the one by Kirk in 1906, only sixteen articles, since 1925, have been used, and those were written by eight men, namely Viau, Blanton, Lindsay, Gies, Waite, Brown, Weinberger and the author. Among these are a number that the author states are of no importance and value; some have been cited many times. Missing from the above list are the names of a considerable number of writers who have contributed valuable material that must be included if the history is to be considered worthwhile.

Is an historian's insistence upon accuracy of data fetishism, or is it nothing more than a spinster's emphasis upon the orderliness of her bureau drawers? Let us see by the following examples, typical of the author's historical research methods. They are but a few, but should supply the answer as to the value of the historian's insistence for accuracy, and whether the " Foundations " can be utilized in a comprehensive and authentic history of dentistry."

1. One wonders how or why the author, on page 982, quotes the following incorrect information from Guerini. He " gives the date of Fauchard's birth as 1690 and his death as 1761. . . . This statement would indicate that his birth antedated 1690 by a number of years." Since 1923, all dental historians know the above to be wrong, that the " Father of Modern Dentistry " Pierre Fauchard, was born in 1678 and died on March 21, 1761. Any of the more recent histories such as my " Orthodontics, etc." 1926, p. 138 and Lindsay's 1933, p. 57, which the author used in Chapter 3 #15, could have prevented such a " faux pas." The August issue of the *Dental Cosmos* in 1923 contains all of the special addresses delivered here at the Academy of Medicine's Fauchard Celebration, while the *Oral Topics*, 1923, pages 1135-1149 likewise has an excellent account of this meeting.

2. Are the " few writers " who discuss " James Daniel, Operator for the Teeth, the first dentist of merit in New York " with " apparent disinterest," wrong, and does he " deserve more respect than has been accorded him? " Page 986. Did Daniels, " in one of his announcements " state " that he practiced these operations in London under March the Surgeon Dentist, a man so eminent in this profession "?

"This suggests that Daniels was probably the first trained dentist to practice in America." As for March, we are informed that he was "one of England's leading dentists of the eighteenth century. . . ." The author's deductions are all very interesting except for the fact that the quoted newspaper does not happen to contain the name of March, as he records, but instead that of a Mr. Marsh, thus an hypothesis built upon such a basis of identity must necessarily collapse by itself. It would likewise be of interest to know where the author obtained his information as to March or Marsh being one of England's leading dentists, for the only record we have is Berdmore's reference to the latter "as ingenious Mr. Marsh." The important fact is that the author here shows a knowledge of Berdmore, a point to be considered when discussing Fendall. Daniel's own advertisement states that he was a "Wig-Maker and Hair Dresser. . . ." "Also Operator for the Teeth, A Business so absolutely necessary in this City . . ." "and that he hopes by this useful Branch of Business, to get more in the Public's Esteem, which is all his Ambition." From this one can readily see that the dental historians have evaluated Daniel correctly.

3. Does Benjamin Fendall, as the author desires his readers to believe, actually "stand at the head of the dentists in eighteenth century America" (page 989) because of "his clear understanding of the science involved in dental practice . . . as indicated by his announcements." The two long and one short advertisement cited by the author are decidedly in contrast to his more popular ones, used from September 4, 1776 through 1786, which are not mentioned. Always headed by the Latin quotation from Berdmore's 1770 edition, and copied verbatim from Baker's 1773 announcement, they remained unchanged during the intervening years, showing that Fendall did not have any original ideas of his own, nor was there any progress in his concept of dental practice. Yet the author uses him as the "yardstick" in the appraisement of others during this period. Had the author studied these announcements more carefully, he would have discovered both to be a case of flagrant literary plagiarism. The broad claims in the opening and closing statements of Fendall's 1784 announcement are not in keeping with the more conservative expression in the remaining portion; they are the opinion of a boastful

individual in contrast to the careful observations made by one well-versed in dentistry. As the author has shown under Daniel that he knew Berdmore's writings, how then did he fail to recognize Fendall's announcement to be nothing more than a verbatim copy of Chapter 1 of Berdmore's, plus that of Baker's? Why does the author repeatedly condemn the transplanting of teeth by others such as " he seems to have ridden in on the tide of popularity of the practice of transplanting teeth " (Le Mayeur, p. 997), yet in Fendall's case he fails to inform his readers of his use of this method of dental practice.

4. If history is to be of value, it must be based upon accurate information, freed from the imaginative, and must be logical. Is the author justifyed then in stating: " His (William Pitt Greenwood) announcements continued on a very high plane until he appears to have entered into bitter competition with Josiah Flagg, which caused Tom Paine, editor of the Federal Orrery, Boston, to print a defense of Flagg placed immediately over Greenwood's announcement in the same paper " (p. 994). Proof is lacking that it was Greenwood who happened to be the " enemy " and no evidence is given to substantiate the author's unjust deductions. What is of far greater importance is that no advertisement of Greenwood, or any other dentist, can be found on page 345 of the Federal Orrery for August 17, 1795. Greenwood's notices ran from June 18th to July 27th and from August 31st again for six weeks; thus Paine's remarks could not have been " placed immediately over Greenwood's announcement in the same paper." This is just one of numerous examples of the author's historical research methods.

5. The reader himself could easily determine how incorrect and untruthful are certain conclusions reached by the author, were he to refer to the original article under discussion, but then how many will take this trouble to verify statements? Advantage is therefore taken of this situation to condemn the efforts of others. Throughout the " Foundations " one finds that the author endeavors to confuse his readers by placing false interpretations upon the writings of others, and offers explanations that the original writer never thought of nor had in mind. A good example of this method of historical research is the author's discussion of LeMayeur, pages 997 and 998. He

writes: "Jean Pierre Le Mayeur has received much more attention from dental historians than his qualifications as a dentist deserve. His prominence may be attributed to the erroneous belief that Le Mayeur instructed James Gardette and Josiah Flagg in the dental art. An analysis of the quality and character of operations performed by Le Mayeur and Gardette, based on their respective announcements in 1784, shows the latter's superior qualifications, and leads to the ready conclusion that the claim is not true. The legend is discounted by George Vieu (10) (spelled incorrectly for Viau), while B. W. Weinberger (11) presents convincing evidence contradicting the claim. Were it not desirable to correct the prevalent belief in Le Mayeur's unusual professional qualifications as a dentist his name would not appear in this narrative." "Le Mayeur's contributions to eighteenth century dentistry were unimpressive and of no effect." I happened to be the "dental historians" who presented to the profession the current information on Le Mayeur in the *Dental Cosmos* for May of 1934. Under "This Issue at a Glance" one will find the following: "The author here has devoted much time and energy to solving a problem that has long puzzled historians of dentistry, namely, the identity of the Le Mayeur who came to America, and the proper spelling of his name. The description of his method and procedure and the enormous number of periodicals, newspapers, etc., he reviewed in his search, constitute an example of effort at accuracy in historical detail that may be well emulated by all dental historians."

The article in question settled the following facts: That it was not a Joseph Jean François Le Maire who came to this country from France; instead it was a Jean Pierre Le Mayeur who practiced surgery in London prior to his arrival in America, that he did not arrive here on July 12, 1780 with Rochambeau, as previously recorded, but in November of 1781; he was not an intimate of Lafayette; that Le Mayeur was not in Newport in 1781 or 1784 "in the thick of the fight bravely opposing the British forces" and "teaching Gardette or Flagg," but was in New York with the British until Washington issued him a pass to travel to Newburg to look after his teeth. He did not return to France where in 1812, 1816, 1821 or 1824, he wrote books accredited to him, and that he did perform professional services for Washington. In clearing up these

points, there was no need to dwell upon his " unusual professional qualifications as a dentist," an interpretation placed upon it only by the author, and which does not aid historical accuracy.

There is likewise no way of comparing " the quality and character of operations performed by Le Mayeur and Gardette, based on their respective announcements in 1784 " or after. Gardette's notices were long and detailed; Le Mayeur's, except one, were but a statement that he was in town, or an offer to buy human teeth for transplanting, the method of that day, and nothing more.

As to the statement " but there is no convincing evidence to prove the latter (Le Mayeur) served Washington professionally " etc., this is only an attempt to discredit the article. On page 574 of the *Dental Cosmos*, 1934, will be found the following: " *Smith's reply was evidently satisfactory to Washington, for on June 20th I find Le Mayeur had just visited him at his headquarters in Newburg, having performed the services desired.*" (Italics not in original.) This information is in a letter from Washington. From that date (1783) until November of 1788, Le Mayeur attended Washington, according to many letters, and contrary to the author's endeavor to convey a different opinion. Again, " one can infer from the diary entries and letters that Le Mayeur's prolonged visits at Mount Vernon were not strictly social, for a week later, June 23rd, 28th, and July 1st (1786) he is mentioned being again with Washington " (p. 576).

The author seems to have the tendency of drawing upon his imagination for facts, judged by the above conclusions. Had he Washington's letter of February 22, 1784, he would have discovered the following: ". . . I received great pleasure from the account which you have given me of Doctor Le Mayeur's operation on you, and congratulate you very sincerely on the success . . . of the efficacy of transplantation of living teeth, etc." " If contrary to expectation this letter should find Doctr. Le Mayeur in New York, let me pray you to present my compliments to him . . . and that I shall whenever it is convenient to him, and not before, be very glad to see him . . . where he . . . will meet a cordial and welcome reception." For five years after this, Washington did avail himself of Le Mayeur's professional service.

Regarding the lack of record of payments, as mentioned by the author, on page 576 of the *Dental Cosmos* article there is the following: " There appears no entry for dental services for himself during this period. As entries are made for Drs. William Baker, Spence, Whitlock and John Greenwood, it might reasonably be presumed that Le Mayeur made no charge to Washington, giving his services in return for the many kindnesses shown him, otherwise some definite amount paid by Washington would be recorded." Where, in the above, can one find the conclusion that " this evidence suggests that Le Mayeur was not employed "?

As to my having lost sight of Le Mayeur after 1788, the author received a letter from me under date of June 24, 1937, which he acknowledged, and in which I wrote: " I finally succeeded in obtaining the Will and some other interesting material of Le Mayeur, and find . . . that he died in this country . . . in May of 1806," etc.

On the whole the author's endeavor to misinterpret my statements proves " unimpressive and of no effect."

6. Earlier in this review I mentioned the feud between Harris and Hayden and as a result the author's inability to free himself of its influence so as to write an unbiased history of dentistry. For that reason he tries to dismiss in one paragraph, by the use of a footnote, the influence on and contributions to dentistry by John Harris " The Founder of American Dental Education." " A careful study of the evidence leads to the conclusion " by him that " there is nothing in dental history to show that John Harris exercised any influence on the progress of either the science or the art of dentistry." This opinion is one that is held only by the author, as can readily be seen in two contributions on pages 872 and 899 in the same " Proceedings." The June issue of the 1941 *Dental Items of Interest*, pp. 517-536, and the July *Journal of the American Dental Association*, pp. 1179-1192, supply further evidence of the author's inability to see the truth. What about the latest evidence the author has uncovered that leads him to his final conclusion? " The Registrar of Transylvania University reports that John Harris was enrolled there in 1835-36 as an undergraduate medical student . . ." Therefore the University would hardly be likely to have him lecture there, etc. Had the author himself visited the medical libraries in Baltimore he might have found

in the " Catalogue of the Officers and Students of Transylvania University " for 1836 the name of one, John M. Harris of Richmond, Kentucky, whose preceptor was a Charles J. Walker, M. D. John Harris, without the middle initial " M " practiced in Louisville, Kentucky, from 1834 to 1840, and therefore could not have matriculated from Richmond. In no single advertisement, articles in journals, obituary, Masonic records, and as the first man to be elected to membership in the American Society of Dental Surgeons, does the middle initial " M " ever appear. The author's latest evidence and then deduction must therefore be discarded along with the others he produced at intervals.

Can an historical study be considered authentic or complete when, on page 867, the author attempts to convey in his discussion of oral infection that the plagiarist, Fendall, understood " the effect of oral disorders on general health "? So far no one has credited Berdmore with a knowledge of this, but by changing Berdmore's " deficiencies " to " diseases," the author reaches the conclusion that Fendall did. How much better it would have been to record history correctly and to have informed his readers that John Harris' dissertation on " Toothache " in the *American Journal of Dental Science*, 1845, pp. 60-110, was the first, in our dental literature, to issue a warning on the effect of pulpless teeth on the patient. The warning in this article was " forgotten " until William Hunter, in 1910, again aroused the medical and dental professions.

7. Individuals read history differently, but that does not relieve one from the duty of evaluating and reporting his material correctly, especially bibliographical references. In his relationship with his colleagues, one at least expects professional etiquette. Let us examine pages 1014-1015, and see how the author himself becomes confused at the same time misleading his readers as to the value of his contemporary historians' efforts. He writes: " The Society of Surgeon Dentists of the City and State of New York is the only one of the local and state societies founded anterior to 1840 of which we have accurate knowledge. The beginning, the purposes and the achievements of this Society were discussed extensively by L. Parmly Brown, first in his 'New Lights on Dental History' (27) and later in 'The First Dental Society and Its Background.' (28) B. W. Weinberger in

'The Origin of Organized Dentistry' (29) also gave an interpretation of its value to dentistry. Of these, 'New Lights on Dental History' is the most reliable and valuable to the dental historian; the other two were written at a time when the authors seemed to be called upon to overemphasize the importance of the subject in order to do justice to the occasion for which the articles were written." The "occasion" although he fails to mention it, was the Centennial Celebration in New York in December, 1934, of the organization of the first dental society. In honor of the occasion the December 1934 issue of the *Dental Items of Interest* was devoted in a large part to papers relating to the Centennial. Brown's "(28)" "The First Dental Society" etc., appeared on pages 825-851 and was written in the careful and accurate manner one expected from him. On pages 852-872 appeared an article "The First 100 Years of American Dentistry" by the reviewer, from which the author did obtain and use material in the "Foundations" without due credit. The "Origin of Organized Dentistry" No. 29 in the bibliography, was not published until November, 1937. Thus the author not only confuses the titles of the two papers, the year of publication and the bibliographical references, but thereby condemns both papers as not being reliable and valuable to the dental historian.

The story of the Greenwoods, pages 992-994, being based upon published material printed years ago must be entirely revised; unfortunately space will not permit me to correct the many many errors and false deductions printed.

* * *

In the face of such careless writing, misleading and unwarranted deductions and handling of the subject, what of the historian's responsibility to his readers, and what of his respect for the history of his subject? Would the physician or dentist be so easily forgiven or excused for such wrong diagnosis and treatment of his patients? Then should the historian? Would the department of history of the University of Maryland encourage such historical research? Why then in the School of Dentistry?

The danger of the careless handling of historical data has been frequently stressed in medical as well as dental circles. However, the

historian has duties other than the careful handling of his materials. He must capture the past for his reader and bring it to life. For a particular profession such as the dental profession, he must establish its kinship with the past by stimulating the feeling and providing the understanding that " the heritage of the past is the seed that brings forth the harvest of the future," p. iii. If the author had succeeded in these things, the reviewer would have felt obliged to refrain from criticizing the article " Foundations." Unfortunately, however, the author's approach to individual early American dentists is seriously colored by his own particular likes or dislikes. One feels that he likes and admires Benjamin Fendall and dislikes and resents John Greenwood. The reviewer is not pleading for an ivory-towered, dry-souled, " impartial " historian who superciliously observes a period without ever actually entering into it. Parrington in his foreword to his " Main Currents of American Thought " observes that " unfortunately, the *mens aequa et clara* is the rarest of attributes, and dead partisanships have a disconcerting way of coming to life again in the pages of their historians. That the vigorous passions and prejudices of the times I have dealt with may have found an echo in my judgment is, perhaps, to be expected; whether they have distorted my interpretation and vitiated my analysis is not for me to determine."

Naturally an historian who truly enters a period also enters its conflicts and understandably becomes a partisan. But because of his knowledge of all or most of the facts involved, as revealed by the available historical materials, he becomes almost a contemporary partisan. Let us see whether the author is such a partisan.

The author writes " to show the scope of his work (Benjamin Fendall), the character of the man and his professional growth we submit this announcement from the *Maryland Journal and Baltimore Advertiser*, July 23rd, 1784." This advertisement contains the following:

> It is not solely by Lucre, or pecuniary Views, the Doctor (Benjamin Fendall) is prompted to offer his assistance to the Public; he is urged by a desire to serve them; kind Fortune has made him independent, and placed him far above the hungry Tooth of Want.

" Benjamin Fendall stands at the head of the Dentists in 18th Century America," the author says of Fendall. One cannot help

speculating as to what Washington thought of Fendall's desire to serve the Public. Would Washington have placed him at the head of the dentists in 18th Century America? This he did not do when he sought the services of Le Mayeur and John Greenwood.

In a letter to Fendall, George Washington writes:

> Mrs. Washington has been long in expectation of receiving what you took away unfinished . . . and I must apply elsewhere, if not done.

to which Fendall replied a year and a half later, excusing himself for being ill (for a year and a half?):

> They are . . . as nearly as I can now recollect—like the old ones . . . the model I took, has also, by accident, sustained some injury . . ." But, in spite of this he closes the letter—" Gen. Washington by B. Fendall . . . Twenty-eight Dollars, which if the Gen'l. chooses—he can enclose me by my Servt. John—Then ce' (receipt) the above in full.

Of John Greenwood, the author writes as follows: " By 1791 we find John making strong claims of superiority: ' Impossibilities must not be expected from him; yet he hath and will perform more than has been by any foreign genius whatever in the line of his profession.' (*Daily Advertiser*, New York, July 5, 1791). He says also that ' He has a peculiar method of fixing artifical teeth, which are not equalled by any other teeth, as to beauty, firmness or durability.' (*Weekly Museum*, New York, July 30, 1791). But Greenwood does not explain what this ' peculiar method ' is. His caustic jibe at ' foreign genius ' and his claim for discovery of ' peculiar methods ' suggest the possibility that he was in competition with other dentists against whom he regarded it desirable to promote himself by the doubtful claims of superiority."

Personally I cannot see how anyone who has studied and understands Greenwood, can see a " caustic jibe." But if the author does, he should have provided some basis for his opinion. For example, he could have pointed out that Greenwood was a patriot, a veteran of the Revolutionary War, living in a period of which Beard [5] says " the war for independence has left a legacy of emotions. Basically a sudden dislike of Britain and things British was unavoidable among

[5] Beard, Charles A. and Mary R., *The Rise of American Civilization*, New York, 1930, p. 463.

the patriots. . . . Of kindred necessity, a consciousness of national independence, and of the challenge which responsibility carried with it forced upward feelings of belligerence and pride."

If the author, even without understanding John Greenwood, really understood Greenwood's time, a period in which American privateers [6] were being christened " Resolute," " Revenge," and " Retaliation " he would not have had to seek for mean and petty motives in Greenwood in order to explain the supposed " caustic jibe at foreign genius."

Leikind [7] in a recent article, divides historians into bead collectors and bead stringers. But, in all due fairness, it cannot be said that the author has either collected new gems of dental history or has strung old ones in new and interesting patterns. His contribution is revealing—but not of the " Foundations of Professional Dentistry."

[6] *The American Revolution in New York.* University of the State of New York, 1926, p. 147.

[7] Leikind, Morris C., *Journal of Bacteriology*, 1939, pp. 485-493.

WEINBERGER AND HIS REVIEW
OF
THE FOUNDATIONS OF PROFESSIONAL DENTISTRY

J. BEN ROBINSON

I do not seek or desire controversy. But I want it understood once and for all that I will, whenever there is point in so doing, comment as I deem proper upon whatever opinions may be advanced in reference to dental history, and will write freely on any part of it that I may choose to discuss. I shall make what contributions are within my power without considering myself at all obliged to defer to those who arrogate to themselves the role of authority, or to enter into defense of my criticisms or my opinions in the event any supercilious disputant elects to assail me. I shall depart from this policy in this instance only to show once and for all the true character of the pseudo critic who has seen fit to assail so intemperately a section of the *Proceedings of the Dental Centenary Celebration.*

I am fully aware that a writer of history bears a great responsibility to the reader and to the subject under discussion which should prompt him to exercise due caution in the use of data and in drawing conclusions from these. But it is also true that the *critic* has still greater responsibilities of which he should always be conscious; and only those who are aware of such responsibilities and who possess superior knowledge, broader understanding, greater discrimination and intellectual honesty should presume to pass judgment on the contributions of others. The review by the critic should aim to emphasize truths and to correct error on a plane of refinement and self-respect, and to avoid resort to disparagement, vituperation and the venting of spleen. The philippic which we have just witnessed violates all standards of fair judgment and respectful criticism. It is not a review by a competent critic setting forth in clear and convincing language important errors; it is deliberate diatribe by a biased partisan in which the utmost confusion is introduced into the picture. It contributes nothing to dental history or to the esteem in which dentistry deserves to be held.

In pondering Weinberger's reasons for making such a bizarre attack it is interesting to consider his dramatic excuse that he is impelled by a lily-pure devotion to his profession to come to its defense at a time when, in his opinion, its very substance is threatened by my *Foundations of Professional Dentistry*. An examination of this excuse is enlightening: "The profession accepts it in confidence alone, and in 'reviews' praises it because of the author's position and standing in the profession. It now has become the foundation for further articles and study, with all of its errors, misstatements and unwarranted deductions to be repeated. For that reason I feel it is my professional duty to bring to the profession's attention some of the mistakes, and show how it was not 'written in the light of the best available knowledge' as claimed. . . ." If this writer, as admitted by Weinberger, has "position and standing in the profession" it would appear that he has earned its esteem; and if reviewers have spoken flatteringly of the *Foundations* it is because they have found in it much that appeals to intellectually honest students. Weinberger's unintentional compliment suggests that my standing in the profession entitles me to speak on the subject without any urgent obligation resting on him to indulge in unbecoming diatribe in an attempt to refute what I have to say.

We may pursue this thought a bit further by inquiring into Weinberger's estimate of his own qualifications which, in his opinion, justify him in contradicting the favorable estimate placed on the *Foundations* by many creditable reviewers. Is it not probable that if all the glaring errors attributed to the *Foundations* by Weinberger are true, then others, no less well informed, even more highly regarded and equally loyal to their profession, might have detected some of the many faults so laboriously enumerated by Weinberger—at least to the extent that their comments might have been less generously bestowed? And would not some of these capable reviewers have warned the author of these grievous blunders instead of giving voice to the many laudatory comments that have not only been contributed to the press but expressed in personal letters to him? Weinberger is not justified in flattering himself that he has qualifications which other writers, even more competent than he, lack.

An inspection of Weinberger's past record reveals that his current

lament and show of determination to rid dentistry of its imagined danger, imminent in the *Foundations*, is but a continuation of his persistent effort to discredit any one who challenges his design to preempt leadership in the field of dental history. This latest episode reminds us strangely of a similar circumstance in 1933 when he became irrepressibly agitated at Dr. L. Parmly Brown and me when we had the temerity, as he interpreted it, to express opinions on certain matters of dental history. His current breast-beating harangue about the threat I now offer to the reliability of dental history is but the echo of an identical wail with which he introduced his tirade against Brown and me in 1933.

Despite the fact that Weinberger elects himself the champion of what he regards as an endangered profession it is obvious that there is a lingering doubt in his mind whether he will be accepted unquestionably as its champion; so in order to reassure the reader of *his* reliability, he says, " Having had published since 1924 some twenty-five papers in both medical and dental journals that related to the subjects under consideration in the *Foundations*, I believe I am in a position to judge what is history and what is fiction." This is a very interesting admission and claim. Weinberger fails to recognize that the mere multiplication of words does not confer on one the essential qualifications of authority, and that verbosity alone does not qualify one to judge the accuracy and quality of serious writing. Nor does he seem to realize that the qualitative element in composition is a more important consideration in judging reliability and authority of the written word than the mere quantitative factor. In view of these facts should we not inquire how much of the " twenty-five papers " so boastfully paraded is *wheat* and how much is *chaff*?

In order to throw some light on this question let us turn to his " published papers " and select one of the twenty-five as an example of the quality of the group. I elect to choose *The Origin of Organized Dentistry* [1] since it may be regarded as a fair example of his writings. A casual reading of this paper readily suggests that had Weinberger limited his voluminous writing to a small fraction of his boasted output the profession would have lost nothing and his vanity would not have suffered so severely. In all the annals of dental literature there is nothing bearing on dental history so inaccurate, so misleading, so

untruthful, so poorly presented as *The Origin of Organized Dentistry*. There is nothing of quality in it or in any of the others to which he points, of such merit and distinction that would confer on him the title of *authority*. Weinberger will not be permitted, on the flimsy pretext of self-appointment supported only by the multiple garrulous effusions to which he points with so much gusto, to validate his insistence that he should be permitted to act as the supreme tribunal before whom *all* matters on dental history shall be tried.

I maintain that Weinberger wrote his so-called "Review" merely as a continuation of personal attacks on me and others begun a number of years ago. During this period he has offered a number of personal thrusts that now culminate in the burlesque that he stages. His all-consuming desire to purge me first appeared almost fifteen years ago and his zeal to hinder my efforts has continued to grow in intensity and arrogance until he has lost all sense of proportion.

His attacks began as a consequence of his ardent support of an effort made in 1929 to have the American Dental Association place its stamp of approval upon an obscure dentist whom he attempted to establish as the "Father of Dental Education."[2] A resolution to memorialize this individual was presented to the House of Delegates of the A. D. A. and was passed without opposition. The President of the American Dental Association appointed a special committee to study the recommendation and to suggest to its Board of Trustees a suitable memorial. After careful study it became clearly apparent that the American Dental Association would commit a serious blunder by supporting this alleged claim and by so doing confer on a doubtful incident the high sanction of organized dentistry's approval. The majority of the Committee reported this conclusion to the Board of Trustees. Weinberger, incensed at the committee's action, appealed to the Trustees through its President in these words: "I trust the recognition of Dr. John Harris will be given thorough consideration by this Association and I would like to suggest that the old Committee, of which Dr. Robinson is a member, be discharged, due to his personal interest in the matter."[3] Weinberger's impudence was ignored by the A. D. A. The Committee was *not* discharged; the Board of Trustees was satisfied with the Committee's report; and the memorial project was abandoned by the A. D. A., to the discomfiture of Weinberger.

Weinberger, smarting under this defeat, bided his time for revenge. He found his first opportunity to retaliate when in 1932 an article of mine, *Dr. Horace H. Hayden and His Influence on Dental Education*,[4] appeared concurrently with Lawrence Parmly Brown's *John Harris: A Conservative View of His Place in Dental History*.[5] These articles gave Weinberger opportunity to use alleged discrepancies therein as valid excuse to attack Brown and me maliciously. In his customary role as the self-appointed guardian of dental history he tried valiantly to mete out punishment that he thought would discredit us among the profession and would serve, he hoped, to remove us permanently from the field of historical scholarship. In his eagerness to do a thorough job of purging he referred to my article in his customary manner: "it contains so many misleading theories that unless attention is now called to them, dental history will in the future likely be based upon them. *I* [italics not in original] shall therefore deal with the facts in considerable detail." He spoke of Brown, who also had challenged Weinberger's "pet theories," with finality and the same condescending superiority: "I shall therefore not go into detail as far as Brown's article is concerned. Here I find the same looseness in presenting evidence and conclusions based upon lack of evidence, as well as 'abouts,' 'probabilities,' etc."[6] His polemic, instead of silencing Brown and me, offered us an opportunity to present convincing evidence in rebuttal[7,8] that established us firmly in the esteem of the profession, and served also to show clearly that Weinberger is not the dependable writer, the infallible researcher and the superior personage that he recommends himself as being.

Weinberger continued, through correspondence, to complain of my writings, particularly my "pet theories" and "pre-conceived ideas," as he pompously referred to them. But except for an insipid. pointless reply to Brown and me, he did not venture soon into the open. Among his undercover gibes was his comment on my article, *The Beginnings of Institutional Dental Education in America*.[9] When he read this offering he became so incensed that in a letter, under the date of December 15, 1937, in which he paradoxically offered insult at the same time he begged me to give him support in an effort to have the A. D. A. publish his history, he said: "Your

recent article in the Journal of Dental Education also indicates a definite attempt to place me in an embarrassing position." Weinberger's name did not appear in the article mentioned; none of his material was used and none of his peculiar theories as such was given the least attention. However, in my discussion of the founding of the Baltimore College no less than six specific traditional beliefs relative to its beginning, which had been concurred in by Weinberger and others, were set at naught. The fact that I pointed out these errors in contradiction to *his* opinions angered him to the point where he ventured to offer me insult in the same breath in which he asked me to do him a favor.

Finally, when the *Proceedings of the Dental Centenary Celebration* came from the press the *Foundations* enraged Weinberger beyond any possible restraint. In his extremity he threw discretion to the wind and pleaded among his acquaintances to aid him in creating a *committee* (with what authority he did not state) before whom the author should be dragged, tried, and purged. Totalitarian dictators at their worst never showed greater zeal in their efforts to dispose of those who dared question their ill-gotten authority. His smallness is illustrated in a proposal contained in a letter to a mutual friend in which he had the affrontery to designate by name certain persons whom he suggested as peers before whom I should be tried. He has become so callous that it never occurred to him that those named are too well informed and too self-respecting to join him in such an unwarranted and futile enterprise. It was in this extremity, and in the realization that he lacked the support of others, that he began more than a year ago to conjure up the flimsy criticisms presented under the respectable caption " Review."

If the history of the case teaches anything it is that Weinberger has been wrong so often that any future attacks by him upon the literary character of others should be ignored. I could cite numerous instances to show that he has stood valiantly for his " pet theories," only to surrender them grudgingly and resentfully when others have presented convincing evidence of his errors. But I shall not stoop to a brawl which would be necessary were I disposed to contend with him by discussing the involved presentation of details in which he takes refuge. I shall content myself by calling attention to parts of

his " Review " which show that he distorts, that he is petty, that he misrepresents, and that he is unable to distinguish what is evidence.

(a) Weinberger pompously reminds the reader that my comedy of errors would not have occurred if I had listened to his wise admonitions: " One might ask whether the author should have set himself a task of such scope and responsibility when advised by those he consulted in his letter of December 2, 1938, that the time was not sufficient to prepare an accurate and worthwhile history and that they, therefore, could not cooperate." The distortion here consists in misrepresentations of the true character of correspondence that dealt not with the *Foundations* but with a proposed comprehensive history of a century of organized dentistry. My letter dated December 2, 1938, to which Weinberger refers, proposed: " The impending centennial celebration of the founding of professional dentistry (1940) suggests the desirability of writing a history of *one hundred years of scientific dental progress.* [Italics not in original.]" It is to be noted that the emphasis is specifically on dentistry *from 1840-1940.* When replies to the inquiry were in from all who had been approached (none of whom stated they " could not cooperate ") I wrote them: " Early this year I wrote you, among a number of students of dental history, concerning a plan for writing a history of one hundred years of professional dentistry. . . . After the many opinions expressed in the replies . . . the project as outlined in my letter under date of December 2, 1938, is abandoned . . . ." The decision to abandon a history that would cover the last one hundred years of dentistry did not impose prohibitions upon the *Foundations* which was in course of preparation to be used for either an introduction to the proposed history, or as a part of the *Proceedings.* This is but one of a number of instances in the " Review " in which Weinberger uses isolated statements or half truths to distort the picture.

(b) In addition to rampant error in his discussions Weinberger's criticisms frequently are unpardonably petty. As an example, he chides me for referring to the 1786 New York City Directory, ponderously informing the reader that I used an " unrecognized facsimile," inferentially questioning the authenticity of my information. The original of the first directory of the New York series is so rare

that *facsimiles* have been produced for general use, and even these are regarded as in the rare book category. Since a *facsimile* is an exact reproduction of an original, and since the New York Public Library would hardly abuse a trust in serving its users with spurious material, it did not occur to this author that he should be so meticulous as to note in his bibliography that the *facsimile* was used. This " unrecognized facsimile," so-called by Weinberger, was handed me in the Rare Book Room of the New York Public Library. The Librarian assured me that the facsimile is authentic. The original, of which I have been able to locate but one, is so rare it is not available for general use. Weinberger's effort to cast reflections upon this reference is very small, and his petty bickerings about it serve only to confuse the reader. This instance of pettiness is a fair example of much of his laborious fuss about accuracy in detail.

(c) As an illustration of Weinberger's disregard for the truth, I cite the following. Referring to the December, 1934, *Dental Items of Interest*, he says, " On pages 852-872 appeared an article ' The First 100 Years of American Dentistry ' by the reviewer [Weinberger], from which the author [Robinson] did obtain and use material in the *Foundations* without due credit." This is a deliberate falsehood. I did not depend on Weinberger for information, but used material, from original sources, which I have in much more complete form than Weinberger saw fit to present it. Because he uses material readily available to others he takes the arbitrary position that should another use the same source material he may do so only at the risk of being called dishonest. I use nothing of Weinberger's at any time without circumspection and when I do, I always give him credit. One acquainted with his careless manner of using so-called evidence will avoid, as much as possible, use of any of his claims or statements.

(d) My observation of Weinberger's argument convinces me that he is unable to distinguish between evidence and assumption. Let us take his " Example 6 " as an illustration of *his* idea of what evidence is and at the same time present another example of how far he can be trusted to tell the whole truth.

Near the beginning of the " Review " Weinberger refers to the

"Harris-Hayden feud that he [Robinson] himself relates on page 1024." Please note that I did not use the word *feud*—this harsh nomenclature is a product of Weinberger's thinking, and the *feud* is *Weinberger's feud*. Moreover, the reference he cited has nothing to do with *John* Harris whom he has in mind. On page 1024 of the *Foundations* I describe the bitterness that grew up between Horace Hayden and *Chapin* Harris without any thought in mind of, or reference to, *John* Harris. It is difficult to understand how Weinberger could misinterpret this meaning unless he does so intentionally.

Coming to his "pet theory" he complains that *I dismissed John Harris with no more than a meager footnote*! I *did* dismiss John Harris with a footnote and with good reasons. Any one desiring to know the reasons may find them set forth in incontrovertible fashion in *Dental Items of Interest*, Vol. 63, No. 2, February, 1941, pp. 105-129. However, I would not insist that Weinberger accept this evidence or the inevitable conclusions to be drawn from it. He is joined to his idols—I am content to let him alone.

After Weinberger threw loosely into the paragraph "Example 6" some of his opinions, references to my conclusions, citation of two recent articles on *his feud*—all commingled without any semblance of sequence or coherence, he concludes:

> Had the author [Robinson] himself visited the medical libraries in Baltimore he might have found in the 'Catalogue of the Officers and Students of Transylvania University' for 1836 the name of one, John M. Harris of Richmond, Kentucky, whose preceptor was Charles J. Walker, M. D. John Harris, without the middle initial "M" practiced in Louisville, Kentucky, from 1834 to 1840, and therefore could not have matriculated from Richmond. In no single advertisement, articles in journals, obituary, Masonic records, and as the first man to be elected to membership in the American Society of Dental Surgeons, does the middle initial "M" ever appear. The author's [Robinson's] latest evidence and then deduction must therefore be discarded along with the others he produced at intervals.

This involved statement ("Example 6") is meant to convey the thought that Weinberger has discovered *new evidence* which contradicts my conclusion that John Harris was a student at Transylvania in 1835-36—at the time when Weinberger would like to have us believe that Harris taught dentistry there. He intimates that I have confused two characters, John *M*. Harris, the physician, and

John Harris, the dentist. He is more specific in another article: "Can he [John M. Harris] and John Harris be the same individual or is it a case of mistaken identity?" [10] Though he doesn't present *proof* of confusion, he promptly comes to the absurd conclusion that he has presented "new evidence." It is another instance, among many, in which he asks a confusing question, offers no answer, and then defines his own silence as "new evidence."

When I read Weinberger's unsupported claim of "latest evidence" in the A. D. A. *Journal* I at once suggested to a former student, Dr. A. C. Eskin,* who has taken great interest in the Harrises, that he investigate further to ascertain if we had confused John Harris, the dentist, with John *M.* Harris, the physician. Dr. Eskin wrote a letter to the Librarian at Transylvania under date of July 9, 1941, to which he received the following astonishing reply:

My dear Dr. Eskin:

I have not been in residence during the summer and your letter of July 9 concerning Dr. John Harris has been held until my return.

In 1929 I had an extended correspondence with Dr. B. W. Weinberger about Dr. John Harris, dentist. It had been thought that he might have given lectures on dentistry in the Transylvania Medical School, as it is known that he was in Lexington in 1835-36.

Various issues of the Lexington paper carried Dr. Harris' professional card as practicing dentistry. Also *The Frankfort Commonwealth* gave prominent announcement of the fact that he would practice in Frankfort during the session of the Legislature. There were one or two scientific articles of some length in the *Commonwealth.* I found no record of his having delivered lectures at Transylvania. All of this I wrote Dr. Weinberger.

It did not occur to me then, I believe, to look for Dr. Harris' name in the matriculation lists of students. There his name appears *John Harris, M. D. 1835-36, Chillicothe, Ohio.* . . .

The John M. Harris, M. D. 1836 of Richmond, Kentucky, could not have been the dentist. Both Dr. John M. and his preceptor, Dr. Charles J. Walker, were well known Madison County men.

More than once we have had these records assembled for some local dentist to examine but none has come. Please do not hestitate to ask any

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*Weinberger is much disturbed because I employ the services of others to assist me in my researches. If his criticism is a valid one, research in general would seem to be in need of much reform.

further questions. We are as eager as you to get the John Harris record in Kentucky established.

Regretting sincerely the delay of this reply,

Cordially yours,

Mrs. Chas. F. Norton, Librarian.

This is a typical illustration of how far Weinberger can be trusted to deal with the truth. I have stated that John Harris, the dentist, matriculated as a medical student at Transylvania in 1835-36; Weinberger insists that it is a case of "mistaken identity."—Mrs. Norton's letter supports my statement. Weinberger has insisted that John Harris taught dentistry at Transylvania in 1835-36; I have contended there is no proof of this.—Mrs. Norton's letter confirms my contention. Weinberger attacked my conclusions on the grounds that I have confused John Harris, the dentist, with John M. Harris, the physician. He assumed a case of "mistaken identity" and dignified *his assumption* by calling it proof that I was in error. Mrs. Norton's letter reveals that there was *no* mistaken identity, and that John Harris *was* enrolled as a student in 1835-36. Weinberger, according to Mrs. Norton's statement, had explored this source of information. Did he fail to explore the source thoroughly, as he insists should be done in the interest of accuracy; or, having explored the source thoroughly, did he withhold vital information which, if known, would have been fatal to his "pet theory"? In either case his unreliability as a writer of dental history is clearly exposed.

And this is the man who prates loudly about the "ethics of the profession"! This is the garrulous disputant who comes to the rescue of the profession out of a pious sense of ethical duty! And this is the man who would attempt to injure me by setting aside valid conclusions by the ready use of an inferential claim of "mistaken identity."

I shall close this distasteful task by quoting the concluding paragraph of my response in 1933 to a similar diatribe by Weinberger:

"Because he [Weinberger] ventures to speak authoritatively in the absence of intimate knowledge of his subject, it is not to be wondered at that he refers to facts as theories and makes an attack that is wholly unjustified and unworthy

of one who has enjoyed a creditable reputation as an investigator in this field. I do not share the fear that future generations will be misled by errors committed by the twentieth century dental historian. The modern student is imbued with the spirit of discovery and what we as historians are producing today will be put to the test. 'Truth crushed to earth will rise again,' and if we have erred in these difficult studies, the future will be no more severe than to regard our failures with a charitable smile." [8]

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MEDICO-HISTORICAL NEWS AND ACTIVITIES

AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE

Preparations are being made for the *Eighteenth Annual Meeting* which will be held in Atlantic City, May 3-5, 1942.

The *Fielding H. Garrison Lecture* will be delivered by Dr. Francis R. Packard on "Medical Case Histories in a Colonial Hospital."

At the *Dinner Session* Dr. Jabez H. Elliott will deliver his *Presidential Address* and, in commemoration of the centenary of ether anesthesia Dr. Hugh H. Young of Baltimore will give an address on "Crawford W. Long: The Pioneer in Ether Anesthesia."

A *Symposium on Galileo* will commemorate the 300th anniversary of the death of Galileo. Dr. Arturo Castiglioni (New Haven), Dr. Leonardo Olschki (Boston), and Dr. Russell L. Haden (Cleveland) will discuss various aspects of Galileo's life and work.

The *Annual Meeting of the Council* will take place on Sunday afternoon, May 3. The *Business Meeting* of the Association will be held Monday afternoon, May 4, and the *Osler Medal* will be awarded for the first time.

A great variety of papers will be presented at the Monday and Tuesday morning sessions so that we can look forward to a very inspiring meeting.

CONSTITUENT SOCIETIES

The Section on Medical History of the College of Physicians of Philadelphia

A meeting was held on March 9, 1942 at which Col. Edgar E. Hume presented a paper on "Scientific Accomplishments of the Medical Officers of the U. S. Army."

The Section on the History of Medicine of the Richmond Academy of Medicine

The Section on the History of Medicine held its annual meeting on March 10, 1942 in the auditorium of the Academy. Two papers were presented:

"The Doctor Looks at Falstaff"
Dr. Walter E. Vest, Huntington

"Medicine in the Poems of Chaucer" (Illustrated)
Dr. George W. Corner, Baltimore

Illustrations showing the development of Obstetrics were on exhibition in the Board Room.

Committee of Medical History and Archives, the Ohio State Archaeological and Historical Society

The Committee of Medical History and Archives will hold its Fourth Annual Meeting on April 1, 1942. The program includes the following papers:

Continuation of Period 1835-1858

"Contributions of Ohio Physicians to the Mexican War"
Dr. Leon Goldman, Cincinnati, Ohio

"The Influence of Samuel Robinson upon Ohio Medical Practice"
Dr. Philip D. Jordan, Oxford, Ohio

"The Introduction of the Microscope in Ohio"
Dr. Russell L. Haden, Cleveland, Ohio

"Outstanding Medical Men in Ohio Masonry"
Dr. James J. Tyler, Warren, Ohio

"The Formation of the Eclectic School of Cincinnati"
Dr. Ralph Taylor, Columbus, Ohio

"Summary of the First Cholera Epidemic in Ohio and the Story of the Second Epidemic in Ohio"
Dr. Edwin W. Mitchell, Cincinnati, Ohio

"The Dental Schools of Ohio Between 1835-1858"
Dr. Edwin C. Mills, Columbus, Ohio

"Contributions of Ohio Physicians to the Natural History and Geological Surveys of Ohio"
Edward S. Thomas, Columbus, Ohio

"The Thomsonian Recorder of Columbus and Alvin Curtis, Its Editor"
Dr. Jonathan Forman, Columbus, Ohio

The New York Society for Medical History

The Society held a meeting at the New York Academy of Medicine Building on March 5, 1942. The paper of the evening was "Notable Contributors to the Knowledge of Syphilis" by Dr. Herman Goodman.

The Society will hold its April meeting on April 2, 1942 with two papers:

"Dentistry in America up to the Middle of the Nineteenth Century"
Dr. Bernhard W. Weinberger

"Early Concepts of Dental Caries in America"
Dr. Louis L. Schwartz

NATIONAL NEWS

Ohio State University, Columbus, Ohio

A course on medical history was offered to the freshman class of the College of Medicine at which the following subjects were presented:

January 8 Dean L. L. Bigelow, Medicine in the Egyptian and Pre-Hippocratic Era.
January 15 Dean Bigelow, The Hippocratic School at Cos.
January 22 Dean Bigelow, The Beginnings of Scientific Medicine.
January 29 Dean Emeritus Upham, Medical Quackery.
February 5 Dean Upham, Medical Cults.
February 12 Jonathan Forman, M. D. Chairman, Committee on Medical

History and Archives of the Ohio Archaeological and Historical Society, Editor of *The Ohio State Medical Journal.* Medicine in Ohio Pioneer Days, the Pioneer Doctor of Ohio, His Origin, His Education, His Equipment, His Drugs, His Technique, His Position in the Community.

February 19 Dr. Forman, The Cholera Epidemics of Ohio. Special emphasis on the first great one and its course in central Ohio.

February 26 Dr. Forman, A Sketch of the 108 Years of the College of Medicine at Ohio State University and Its Predecessors.

March 5 Dean Upham, Organized Medicine.

March 12 Charles A. Doan, M. D., Evaluation of The Dance of Death Concept in Art.

Beaumont Medical Club, New Haven, Conn.

The 65th regular meeting of the Beaumont Medical Club was held in the Beaumont Room of the Yale Medical Library on March 13, 1942 with the following program:

"The Peter Parker Collection of Medical Paintings"
Samuel C. Harvey

"Anthony Carlisle—Physician Friend of Joseph Farington"
George H. Smith

NEWS FROM LATIN AMERICA

Argentina

We greatly welcome the appearance of a new serial publication, the first number of which has just come from Buenos Aires. It is the *Revista Argentina de Historia de la Medicina, Organo Oficial del Ateneo de Historia de la Medicina.* The new journal was founded and is edited by Dr. Juan Ramon Beltrán, Professor of the History of Medicine at the University of Buenos Aires. The first number contains the following articles:

Horacio Abascal: Los primeros estudios en Cuba. Fundación de la Universidad.

Juan Ramon Beltrán: Rosas y la Vacuna.

Carlos Martínez Durán: La odisea de Gabriel Ronsil, cirujano y dentista, que ejerció su profesión en Buenos Aires, en el año 1783.

Henry E. Sigerist: La Historia de la Medicina en la República Argentina.

Juan Marín: Notas sobre el "Jade" de China.

It contains in addition a number of notes and comments on medico-historical events and critical book reviews.

BOOK CATALOGUES

Schuman's, 20 East 70th St., New York: Medical Miscellany, List D.